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MIND

A QUARTERLY REVIEW

OF

PSYCHOLOGY AND PHILOSOPHY

I.—OBITUARY

THE Editor regrets to announce the death of Ludwig Wittgenstein on 29 April, 1951, at Cambridge.

II.—LUDWIG WITTGENSTEIN

BY BERTRAND RUSSELL

WHEN I made the acquaintance of Wittgenstein, he told me that he had been intending to become an engineer, and with that end in view had gone to Manchester. In the course of his studies in engineering he had become interested in mathematics, and in the course of his studies in mathematics he had become interested in the principles of mathematics. He asked people at Manchester (so he told me) whether there was such a subject, and whether anyone worked at it. They told him that there was such a subject and that he could find out more about it by coming to me at Cambridge, which he accordingly did.

Quite at first I was in doubt as to whether he was a man of genius or a crank, but I very soon decided in favour of the former alternative. Some of his early views made the decision difficult. He maintained, for example, at one time that all existential propositions are meaningless. This was in a lecture room, and I invited him to consider the proposition: "There is no hippopotamus in this room at present". When he refused to believe this, I looked under all the desks without finding one; but he remained unconvinced.

He made very rapid progress in mathematical logic, and soon knew all that I had to teach. He did not, I think, know Frege personally at that time, but he read him and greatly admired him.

I naturally lost sight of him during the 1914-18 war, but I got a letter from him soon after the armistice, written from Monte Casino. He told me that he had been taken prisoner, but fortunately with his manuscript, which was the *Tractatus*. I pulled strings to get him released by the Italian Government, and we met at the Hague, where we discussed the *Tractatus*. line by line.

I cannot say very much about his opinions before 1914, as they were in a state of formation and flux. He was thinking very intensely and very fruitfully, but was not yet arriving at anything very definite. While I was still doubtful as to his ability, I asked G. E. Moore for his opinion. Moore replied, "I think very well of him indeed". When I enquired the reason for his opinion, he said that it was because Wittgenstein was the only man who looked puzzled at his lectures.

Getting to know Wittgenstein was one of the most exciting intellectual adventures of my life. In later years there was a lack of intellectual sympathy between us, but in early years I was as willing to learn from him as he from me. His thought had an almost incredible degree of passionately intense penetration, to which I gave whole-hearted admiration.

He was in the days before 1914 concerned almost solely with logic. During or perhaps just before, the first war, he changed his outlook and became more or less of a mystic, as may be seen here and there in the *Tractatus*. He had been dogmatically anti-Christian, but in this respect he changed completely. The only thing he ever told me about this was that once in a village in Galicia during the war he found a bookshop containing only one book, which was Tolstoy on the Gospels. He bought the book, and, according to him, it influenced him profoundly. Of the development of his opinions after 1919 I cannot speak.

The Editor hopes to publish in the next issue Memoirs by Prof. G. E. Moore, J. T. Wisdom and F. Waismann covering the other periods of Ludwig Wittgenstein's life.

III.—MR. KNEALE ON PROBABILITY AND INDUCTION

I.

By F. J. Anscombe

As a statistician, whose job is to advise on how to make inductions from observations with the aid of the theory of probability, I hope I shall be pardoned some comments on Mr. Kneale's discussion of this subject.[1] The parts of his book that have been of most direct interest to me are Part III (The Theory of Chances), in which various mathematical theories of probability are discussed, and Part IV (The Probability of Inductive Science), in which the application of probability theory is considered. I will confine my detailed comments to these two parts.

But first I should like to mention a point of emphasis of the book as a whole. Mr. Kneale remarks (p. 158) that " the business of the philosopher . . . is to clarify the meaning of probability statements made by plain men "; and although much of the book is concerned with technicalities that arise in scientific work and not in the plain man's everyday affairs, one is aware all the time that Mr. Kneale's ultimate object is to explain the common usages of certain probability words. My own interest in the subject is more limited. I should like to see clarified the probability statements made by scientists, *i.e.* I should like to know how scientists use probability concepts in their work, and, if possible, how they *should* use them. Once this has been done, the probability statements of plain men should prove fairly easy to describe, since, when not fallacious, they would presumably be found to be approximations to the procedures of the scientists. But so far the probability reasoning of scientists has not, I believe, been adequately described. There has been a great development of the mathematical theory of statistics, which has as its object the facilitating of certain kinds of induction, but no accurate account seems to have been given of the way in which the new techniques should be used.

[1] W. C. Kneale, *Probability and Induction*, Oxford, 1949.

Theories of Probability

The task which Mr. Kneale has undertaken in Part III of his book, that of reviewing the theories of probability, is a difficult one. A sympathetic appreciation of diverse points of view is required, and that is not easily had, since most writers on probability are guilty of disconcerting silly statements about their own work and other people's. It seems to me that Mr. Kneale does not explain sufficiently clearly the difference in intention of the theories he is discussing, and that some of his criticism does not go deep enough. Be that as it may, a rather different rough-treatment of the subject from Mr. Kneale's may prove useful as a supplement.

There are two main sorts of probability theory, those in which the probabilities measure frequency of occurrence, and those in which they measure degree of rational belief (or rational betting odds). At present two main formulations of the frequency theory are being developed, the set-function theory of Kolmogorov and the collective theory of von Mises.

In the set-function theory,[1] probabilities are defined as numbers (additive measure functions) attached to the possible outcomes of an experiment, satisfying certain very simple axioms ; such as, that the probability attached to one or other of two alternative (exclusive) outcomes of the experiment is the sum of the probabilities of the outcomes separately, and that the probability attached to the whole field of possible outcomes is equal to 1. (These axioms are *suggested* by the properties of frequencies of occurrence in a finite series of trials, and are properties that we should expect limits of frequency in an infinite sequence of trials to have, if such limits exist. There is, however, no reference to limits in the axioms.) Next, the word " independent " in the phrase " independent trials " is given a special definition. We now prove various propositions about series of independent trials, and in particular the laws of large numbers. The theory, which so far is pure mathematics, is interpreted with the aid of the rules : (i) if the probability of a possible outcome of a trial is 1, that outcome will be observed, (ii) if the probability is zero, it will not be observed, (iii) certain kinds of trials are " independent ". It is found that this mathematical theory now fits some kinds of phenomena, in "games of chance", genetics, and atomic physics. Characteristic features of the theory are that

[1] A. N. Kolmogorov, *Foundations of the Theory of Probability*, 1933, English translation, Chelsea, 1950 ; J. E. Kerrich, *An Experimental Introduction to the Theory of Probability*, Munksgaard, 1946 ; M. S. Bartlett, *Dialectica*, vol. 3, 1949, p. 104.

the probabilities are thought of as constant and that "independent" trials are always conceived as possible. Hence the laws of large numbers are always applicable, and the probabilities can always be interpreted as limits of frequency of occurrence, in the sense of the laws of large numbers. The theory is therefore referred to usually as a frequency theory. It is the theory which finds most favour among statisticians at the present time.

The frequency theory of von Mises [1] differs from the above set-function theory in the way in which it is started, *i.e.* in its axioms and early development. The basic notion is of a "collective", *i.e.* an unlimited sequence of observations of a random phenomenon; and probabilities are defined as frequencies of occurrence in a collective. In superstructure and method of use, the theory is identical with the set-function theory—or rather, such differences as there are do not result from the axioms—and philosophically there is nothing (except simplicity) to choose between them. Most mathematicians prefer the set function theory, because its basic concepts and procedures are more familiar; in fact, it has been claimed that the notion of a collective is not sufficiently well defined for a rigorous mathematical theory to be based on it. But this is merely a quarrel among mathematicians. A line of attack on von Mises' theory developed by Mr. Kneale (lower half of p. 200) seems to me unfair, and has been answered by von Wright.[2] By "the frequency theory" below, I shall mean either form indiscriminately.

In contrast to the frequency theory, there are theories in which the probabilities are defined as measures of credibility, or as fair betting odds.[3] The principal modern exposition is that of Jeffreys. A probability can be attached to any proposition, and measures a rational expectation of its truth. If the proposition is known to be true, its probability is taken to be 1, if known to be false, 0; in other cases the probability lies between 0 and 1. The theory includes the frequency type of probability as a special case, namely when the proposition considered relates to the outcome of a trial which can be repeated as often as we please and which is known (either from extensive past experience or by hypothesis) to give rise to independent observations of the

[1] R. von Mises, *Probability, Statistics and Truth*, 1928, English translation, Hodge, 1939, and *Notes on Mathematical Theory of Probability and Statistics*, Harvard, 1946.

[2] G. H. von Wright, MIND, vol. 49, 1940, p. 265.

[3] J. M. Keynes, *A Treatise on Probability*, Macmillan, 1921; F. P. Ramsey, *The Foundations of Mathematics*, Routledge & Kegan Paul, 1931; H. Jeffreys, *Theory of Probability*, Oxford, 1939; I. J. Good, *Probability and the Weighing of Evidence*, Griffin, 1950.

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sort described by the frequency theory of probability. Then the rational-belief probability that the proposition will prove to be true is equal to the frequency probability of the outcome in question. But rational-belief probabilities can be attached to any proposition whatsoever, and measure its credibility in the light of the available evidence. They are always known or calculable (in principle), never require empirical estimation, and cannot be verified empirically (except, possibly, psychologically). In general, the probability that an event will be realised in a trial depends on the evidence and changes progressively if successive trials are made.

A rational-belief theory is thus more ambitious than the frequency theory; for not only is it applicable in more numerous situations, but it claims to be a theory of correct methods of thought and not a theory of observed phenomena. The frequency theory is used to describe or explain observations, and so to assist us in drawing conclusions from them; the rational-belief theory tells us directly what we should think. Rational-belief theories can be attacked on two grounds: (i) we may not agree that the axioms of the theory are adequate to describe the thought processes in question, (ii) granting the axioms, we may not agree that the recommended use of the theory is correct. In these matters there seems to be more scope for heated opinion than reasoned argument. Under heading (ii), a notable feature of Jeffreys' theory (as of other earlier ones) is that much attention is paid to interpreting experimental observations when there is no prior information as to the truth of the propositions under consideration. The situation is handled by proposing certain prior probabilities for the various alternative propositions that might be true, the probabilities being chosen to denote complete ignorance. If of three alternative propositions one must be true and we have no evidence at all as to which is true, each is given a prior probability of 1/3. The process is not unique, since if one of the alternative propositions is subdivided into two, or two of them are combined into one, by the same rule we shall get different prior probabilities, and hence eventually different posterior probabilities. Thus a slight alteration in the logical structure of our question, which we feel intuitively is irrelevant, leads to a different answer; and if both forms are used at once contradictions result, as exemplified by Mr. Kneale in his discussion of Laplace's rule of succession (top of p. 204). A similar difficulty occurs in Carnap's theory of probability. To avoid it, arbitrary conventions are suggested. In my opinion, the situation of no prior evidence is one that never occurs in scientific work and

should not be considered. As soon as any proposition or hypothesis has been formulated which is worth testing experimentally, there is already evidence as to its truth derived from existing accepted knowledge and from considerations of analogy or "consilience". A question to which we have no grounds whatever for hazarding an answer is an idle question and would not be the subject of scientific investigation. Unfortunately, when the prior probabilities are chosen in an honest attempt to represent prior evidence, as Good suggests they should be, much of the theory becomes mathematically difficult and too clumsy for use without crude approximations. But viewed as a description of inductive reasoning, and not as a tool for everyday scientific use, Jeffreys' (or Good's) theory seems to me to have only one serious shortcoming, that it omits all consideration of risk or profit. (I return to this point later.)

The range theory of probability that Mr. Kneale himself proposes involves essentially the same definition of probability as in the set-function frequency theory, except that in addition to postulating the elementary properties of probabilities Mr. Kneale gives at the outset a metaphysical interpretation of probability as a ratio of ranges of possibility. While such an interpretation may be held in mind if one likes, it appears to me to be unnecessary. In the set-function theory, no meaning is attached initially to the probabilities; the meaning lies in the use of the theory. We require to be able to recognise situations to which the theory can be applied; when applied, it is a transcendent hypothesis, like an electron theory in physics. Mr. Kneale has pointed out to me that the analogy between the words "probability" and "electron" is poor if the totality of meanings of these words in common as well as scientific speech is considered. But when a statistician, using the frequency theory, quotes the value of a probability, asserting (for example) that some effect is significant at the 5 per cent. probability level, he is alluding to a well-defined transcendent theory, just as is a physicist who uses the word "electron" in describing his observations.

Statistical Methods

The methods that have been developed by statisticians within the frequency theory of probability for the purpose of making inductive inferences from observations are aids to induction, not a theory of induction. Mr. Kneale's paragraph on the meaning of the word "statistics" (pp. 219-220) states this clearly and accurately. But when he goes on to describe some of the "certain

statistical devices that have been introduced specially in order to facilitate the making of inferences from samples ", on pp. 220-222, and when he discusses what an experienced investigator would do who wished to blood-group the population of a South Sea island, on pp. 216-217, the best modern statistical practice is not described at all, but only the practice of fifty years ago or of the most backward present-day Continental demographers. Modern methods do, I maintain, facilitate the making of inferences in a more radical and rational way than Mr. Kneale suggests. The evidence provided by the observations is marshalled in the most cogent form, so that it is most easily assimilated and conclusions may be drawn with the fullest understanding of the way in which the observations support them. Moreover, much attention has been paid to the design of statistical investigations, to planning the number and type of the observations so that the maximum amount of relevant information can be obtained from them.[1]

Induction

The account of primary induction given in Part IV of Mr. Kneale's book seems to me to be over-simplified and therefore in some respects misleading. The word " induction " I take to cover (i) deciding what inferences (*i.e.* statements, in the form of laws, generalisations, theories, predictions of future events, etc.) should be drawn from some observations—in other words, deciding what to think about the observations—and (ii) deciding what actions should next be taken. Sometimes the action side of the induction is the most noticeable, sometimes the thought side. Thus, very commonly the only conclusion to be drawn from some observations is that further observations, perhaps of a different kind, are needed ; but from time to time it will be possible to assert that some hypothesis or law has been established.

In practice inductive decisions are often made quickly and without much conscious reasoning. Nevertheless, it is interesting to try to outline a pattern of fully conscious inductive reasoning. What are the steps in the argument from observations to induction ?

First it is necessary to decide whether the observations are in accord with our existing knowledge, *i.e.* with the corpus of

[1] On statistical methods of handling data see : R. A. Fisher, *Statistical Methods for Research Workers*, Oliver & Boyd, any recent edition ; M. H. Quenouille, *Introductory Statistics*, Butterworth-Springer, 1950. On design see : R. A. Fisher, *The Design of Experiments*, Oliver & Boyd, 1935 ; F. Yates, *Sampling Methods for Censuses and Surveys*, Griffin, 1949.

accepted theory. Of any conceivable results of an experiment or observational enquiry it is (in principle) possible to say whether they are in agreement or in more or less strong disagreement with existing knowledge. (Sometimes all conceivable results of an experiment are in agreement with existing knowledge. In a test of card-sharping ability, it is necessary that all cards produced should appear to be genuine playing cards, and all results recorded should be names of playing cards ; beyond that, prior knowledge tells us nothing, and, for example, imposes no limits to the frequency of occurrence of particular cards. On the other hand, in an experiment to test the effect of an agricultural insecticide, if the crop yields came out ten times higher than a good normal yield, there would be a strong conflict with existing knowledge concerning the action of insecticides.) If disagreement occurs, it may be explained on one of three grounds : (i) a mistake has been made in carrying out the experiment or in recording the observations, (ii) the existing knowledge is wrong and needs modification, (iii) there has been a mistake in deductive reasoning. The first of these explanations is often adopted in some fields of experimental work, *e.g.* in chemical analysis.

If there is no noticeable disagreement between the observations and existing knowledge, so that the observations can be accepted at their face value, we consider the various possible hypotheses that we are interested in in the light of the observations, paying regard to *prior confidences* and *risks*. By " prior confidence " (a term which I prefer to " prior probability ", since I use the frequency theory of probability) I mean confidence or expectation that the truth lies in some range, derived from analogy and previous experience. The degree of confidence may lie anywhere between a mere willingness-to-consider and a very strong expectation close to " knowledge ". If several distinct hypotheses are considered, there will be a prior confidence for each. The confidences are rather vague in relative magnitude, but if one were to try to formalise inductive reasoning there would not, I think, be any obvious inadequacy in postulating a numerical distribution of prior confidence (an additive set function whose value for the whole field of hypotheses is unity). However, since most of us do not think harder than we need, it happens sometimes that the prior confidence to be attached to a hypothesis, *i.e.* the degree to which it is rendered plausible or likely by analogy or experience apart from the observations at hand, is only assessed after the observations themselves have suggested the hypothesis. The experimenter may then perhaps perceive that there is a good

argument for the hypothesis from analogy or previous experience, and he will say, " Why didn't I think of that before ? ". It would therefore be incorrect to suggest that a satisfactory approximation to a complete distribution of prior confidence is actually thought out before the observations are examined. And sometimes again the observations suggest a new hypothesis for which there is no strong prior evidence and which would not have occurred to the experimenter before the observations were available, even if he had thought very carefully about which hypotheses he ought to consider. He notices an interesting unexpected property of the observations, and wonders whether it is generally true or only a coincidence. There has been a good deal of discussion in statistical circles on whether any use can be made of current methods of testing hypotheses in such cases. A satisfactory formal theory of inductive reasoning would need to cope with this situation in some way other than by ignoring it.

By " risks " I mean the expected loss of time or money, or any other sort of loss or disadvantage, if a hypothesis is accepted as true which turns out eventually to be badly out. Expected gain if the accepted hypothesis turns out to be satisfactory can be counted in as a negative risk. It is worthwhile to distinguish different purposes one may have in accepting a hypothesis: (i) to base an administrative decision on, (ii) for further testing and confirmation, (iii) for acceptance into the corpus of scientific knowledge, to be relied on in future work. There are risks, variously assessable, in coming to decisions in all three cases. For example, in case (iii), if the hypothesis is later found to be seriously false a lot of effort in investigating other points may have been wasted. Just as with prior confidences, risks are rather vague in magnitude, but in a formal theory it would be tempting to postulate a complete numerical risk-function.[1]

It has been suggested that risks are more easily assessed, more " concrete " or " objective ", than prior confidences. It appears to me, however, that in ordinary scientific research they are just as personal to the experimenter and just as vague in magnitude as prior confidences ; and no more, and no less, important. Even in the field of observation for which the modern concept of risk function was first developed, namely, inspection of the output of a mass-production line, risks are not easy to estimate accurately, even in principle ; and so far as I am aware no serious attempt has ever been made to cost industrial inspection.

[1] See A. Wald, *On the Principles of Statistical Inference*, Notre Dame, 1942, *Statistical Decision Functions*, Wiley, 1950.

Thus inductive reasoning involves a balancing of evidence provided by the observations at hand, prior confidences in hypotheses, and risks attached to decisions. I suggest that such a weighing up does normally take place when a scientist is thinking about his observations, in the sense that if he were to give intelligible answers to questions on his procedure (such as, " Why do you persevere in trying to establish this unproved hypothesis ? " or " Why don't you follow up such-and-such an alternative line of investigation ? ") the answers could be rephrased in the terms I have just been using. I do not wish to suggest, however, that the considerations necessarily take place in the sequence in which I have mentioned them. If the purpose of the observations is to answer a single clear-cut question, a sequential sampling method may be used. Then the consideration of prior confidences and risks takes place before the observations are made, in selecting the sampling method; and the conclusion follows immediately upon the observations, without further thought.

In his account of the policy of induction (§ 43), Mr. Kneale seems to suggest that the policy consists of following a principle generally known among statisticians as the " method of maximum likelihood ". I conclude by some remarks on the use of this principle.

(1) The maximum likelihood principle is for the estimation of unknown constants (*e.g.* unknown probabilities) occurring in the probability distribution of the observations. Not all induction problems can be framed as problems of estimation, some being, for example, questions of adequacy of fit of a specified theoretical probability model, in which case the relevant statistical tool is not an estimate but a test of significance.

(2) Granted that we wish to estimate a constant, it must be noted that there is an assumption in supposing the " constant " to be a constant. When we speak of $P(\alpha, \beta)$, the frequency-probability that an α thing is a β thing, we are already assuming that β-ness is distributed over α-ness in a way that is adequately described by a fixed probability law. By no means all " random " phenomena can be described in terms of fixed probabilities, and this not because of difficulties in defining α and β. The output of a mass-production machine may contain defective articles that seem to occur in a random manner, but not with a constant probability, so that at some periods the defectives occur rarely and at other periods frequently. Variation in biology is often of this wobbly kind, and the application of probability theory has to be rather crafty. In fact, probability

distributions are applied as an approximate model for small numbers of observations, just as one might represent the path of a planet over a short time interval as a straight line. The approximation works excellently for some purposes.

(3) If we are satisfied that a simple probability model is appropriate to describe the observations, and if we wish to estimate an unknown constant in the model, a single estimate of the constant, such as would be given by maximum likelihood or any other method of "point-estimation", will usually not suffice by itself, since it must be considered in the light of the prior confidences and risks before a rational induction can be made. We require to know how accurate the estimate is likely to be. This need is sometimes met by estimating the error-variance of the estimate, so that we now have two estimates, an estimate of the unknown constant that we are interested in, and an estimate of the accuracy of this first estimate. There are other ways of dealing with the situation. One is, to abandon the idea of a point estimate and evaluate instead an interval estimate, *i.e.* a pair of values known as "confidence limits", as close together as possible, which are such that we may place a known high degree of confidence in the assertion that the unknown constant lies between them.

(4) The method of maximum likelihood is one among many methods of point estimation. It can be shown mathematically to have certain properties which often make it the best to use. In particular, if the number of observations is large and if the total number of unknown constants in the probability distribution of the observations is small (and if the probability distribution satisfies some mathematical conditions that are nearly always satisfied in practice), then the method of maximum likelihood gives estimates of the constants that are at least as accurate as estimates derived by any other method. Note the phrase "at least as accurate". Maximum likelihood estimates are not more accurate that all other estimates, since some other estimates are equally accurate. They are merely not less accurate than others. Note also the condition that the number of observations is large. If the number of observations is small, there are sometimes other estimates that are preferable to maximum likelihood estimates. And note, finally, the condition that the total number of unknowns in the probability distribution must be small. Examples have recently been given to show that if the number of unknowns is of the same order of size as the number of observations the maximum likelihood estimates may be worthless, while other estimates can be found that are

satisfactory.[1] Thus if statisticians frequently use the method of maximum likelihood, they do so because the method is frequently convenient and satisfactory, not because it is necessarily either. Attempts have been made from time to time to give a philosophical justification of the method, showing that it is *a priori* reasonable, without reference to the properties that its estimates can be proved to have. Mr. Kneale gives such a justification (pp. 230-233). I understand that, in view of criticism by Broad,[2] Mr. Kneale would now wish to see this passage modified somewhat; and I will only comment on one detail of it. Mr. Kneale stresses the boldness of the policy of induction, *i.e.* of maximum likelihood estimation. I would maintain that the boldness inheres, not especially in the maximum likelihood method itself, but in using a single point estimate at all (rather than an interval estimate, for example). A point estimate is almost certainly wrong, but in the circumstances when the maximum likelihood method is satisfactory, the maximum likelihood estimate is likely to be as little in error as any point estimate.

The above remarks do not constitute a "review" or "appreciation" of Mr. Kneale's book. I have tried merely to supplement his discussion of certain topics where greater detail seems desirable and in particular to indicate some developments of statistical theory, relevant to the philosophical problems discussed, that have taken place since 1939, when the book was begun.

Cambridge University.

[1] J. Neyman and E. L. Scott, *Econometrica*, vol. 16, 1948, p. 1; A. S. C. Ehrenberg, *Biometrika*, vol. 37, 1950, p. 347.

[2] C. D. Broad, MIND, vol. 59, 1950, p. 94.

IV.—PROBABILITY AND INDUCTION

II

By William Kneale

Mr. Anscombe has dealt gently with the defects of my book, and I shall not waste space by trying to make a detailed defence, but say instead something about a number of points of general interest which he has raised.

(1) *The Scope of the Philosophical Study of Probability.* A philosophical inquiry is usually an attempt to clarify some concept already in use, and when we make such an attempt we must pay close attention to the utterances of the people, whether plain men or scientists, who use the concept. Now the noun 'probability' is much commoner among scientists than among plain men. When plain men use the word, they most often wish to refer to the fact of some proposition's being probable, *i.e.* approvable as a basis for action in most situations. Thus they say sometimes 'We can't be satisfied with mere probability in this matter: we must have certainty'. Scientists, on the other hand, talk of probability as a number in statements of the form 'The probability of an α thing's being β is p'. The second usage dates, I believe, from 1718, when De Moivre defined a probability as a proportion of chances; and if anyone dislikes it, he can avoid it by talking systematically of chances. At one time while I was writing my book I thought of trying to do this, but came to the conclusion that the gain in clarity was not worth the effort.

In a discussion of mechanics it would, of course, be pointless to examine the way in which plain men use words like 'force' and 'energy'. But the theory of probability is not related to talk about the probability of rain on the day of the village fête in the same way as the theory of mechanics is related to talk about the force required to knock a coconut off its stand. When we say in scientific fashion that the probability of an α thing's being β is p, we are not talking about beliefs or making recommendations for behaviour; but our statement is an elaborate version of the kind of thing the plain man would regard as justification for one of his assertions containing 'probable' or 'probably'. Consider for example this conversation: 'He'll probably lose his money.' 'Why?' 'Well, he's beaten unless

his next throw produces a double-six, and there is only one chance in thirty-six of a throw's producing that result.' In short, a theory of chances, or probability in the technical sense, should be free from entanglement with psychology and ethics and economics, but also such that it allows for the use of statements of chances to justify judgments of the kind plain men express with 'probable' and 'probably'. Normally statisticians get on very well without worrying about philosophical questions; but if anyone feels the intellectual cramp which makes him want to ask philosophical questions about probability, I do not think he can safely ignore the usages of plain men. I shall give an illustration of this point later.

(2) *Theories of Probability.* Mr. Anscombe speaks of two theories, one concerned with frequencies, the other with degrees of rational belief, and each respectable within its field. This way of describing matters seems to me unfortunate for two reasons.

In the first place, I do not think that the phrase 'degree of rational belief' has any clear meaning. In ordinary life we sometimes talk of a man's belief as more or less firm (*i.e.* difficult to shake), but we never say 'You believe that proposition more than it is rational to do'. And although we sometimes make remarks such as 'You put more faith in the hypothesis than it is rational to do' or 'You are taking bigger risks than it is rational to take' or 'The fair odds would be less than you are offering', we commonly assume that these can be justified by reference to chances which are independent of anyone's attitude. Mathematicians and philosophers who talk of degrees of rational belief as though this conception required no further analysis reject such explanations. Where, then, do they find the subject-matter of their investigations? At one place in his exposition of their views Mr. Anscombe implies that the axioms of the rational-belief theory describe thought processes, and at another place he suggests that according to this theory the probability of a proposition might conceivably be discovered by a psychological observation. Presumably the thought processes described by the axioms would be processes of a kind called rational, and the psychological observations would be performed on subjects whose beliefs were known to be rational. But how is rationality to be recognized in this context? So long as we are concerned only with the making of a calculus (*i.e.* with patterns of inference from one probability judgment to another), consistency is enough. But the rationality assumed in the rational-belief theory may apparently belong to single probability judgments,

and so it cannot be mere consistency. Is there any way of defining it without going over to another theory of probability ?

Secondly, it seems to me that Mr. Anscombe obscures important differences when he groups all rival views under the name ' frequency theory '. From his explanation it appears that he is prepared to give the name to any doctrine which (*a*) deals with the probability of a so-and-so's being such-and-such and (*b*) allows for the testing of probability hypotheses by reference to frequencies in actually observed sequences. In this large sense almost everyone who has written about the subject has been a frequency theorist in the better parts of his work : even James Bernoulli and Laplace qualified for the title when they talked of estimating unknown probabilities. But persons who agree on the two points just mentioned may yet disagree in their analysis of expressions of the form ' The probability of an α thing's being β is p '. And there are in fact three schools : (i) the frequency theorists in the strict sense, who try to *define* the probability of a so-and-so's being such-and-such by reference to frequencies, (ii) the range theorists, and (iii) the formalists, or exponents of the set-function theory, who say in effect that it is sufficient to define probability implicitly by axioms and rules of application. Mr. Anscombe favours the third school, and I shall try to say something about their tenets (which I neglected in my book).

(3) *The Set-function Theory.* As Mr. Anscombe says, the whole of the mathematical theory of probability can be developed in abstract fashion from axioms which impose only formal requirements. But why should we call this a theory of probability ? Mr. Anscombe tells us that ' no meaning is attached initially to the probabilities ' and adds ' The meaning lies in the use of the theory '. If this is so, there can be no special reason for using the word ' probability ' in the development of the theory, and it may even be wise to avoid the word for fear of dragging in irrelevant associations. Let us suppose, then, that the theory has been developed as one about *P*-numbers, and that these are defined so far only by the axioms. Any interpretation we reach must obviously depend on the rules of application. But what do these add ?

Mr. Anscombe tells we are to assume that if the *P*-number of a possible outcome of an experiment is 1 this outcome will be observed, and similarly that if the *P*-number is 0 this outcome will not be observed. This is some help ; but what about assertions of intervening values ? In conception the frequency theory of von Mises and that form of the range theory which I

have tried to defend are both attempts to interpret such assertions by extension of philosophical theories of natural law, the frequency theory of probability corresponding to the constancy theory of natural laws and the range theory of probability to the necessitation theory of natural laws. The formalists, however, refuse to commit themselves to anything of this kind, and Mr. Anscombe says they regard an hypothesis about the value of a *P*-number as something like an hypothesis about electrons. I find it very difficult to make anything of this analogy. The notion of a *P*-number, unlike the notion of an electron, does not belong to any special branch of science, but rather to the common equipment of all scientists; and although hypotheses about the value of this or that *P*-number may be revised as science progresses, it seems queer to suggest that the very notion of a *P*-number might be modified, as the notion of an electron has been, in the light of empirical discoveries. But let us put that difficulty on one side and consider how a formalist would proceed in testing an hypothesis that a *P*-number had a value of about $\frac{1}{3}$.

Mr. Anscombe says that the assumption of the independence of trials allows us to use the laws of large numbers, and implies that these in turn make it possible for us to test hypotheses about the values of *P*-numbers. But the abstract mathematical theory is concerned only with relations between *P*-numbers, and it does not contain any theorem to the effect that if the *P*-number associated with a possible outcome to a certain type of experiment is $\frac{1}{3}$ the *P*-number associated with the occurrence of that outcome in about $\frac{1}{3}$ of a very large number of trials must be 1. Within the formalism we are considering, Bernoulli's theorem asserts only the convergence of a certain infinite sequence of *P*-numbers to 1, and we have no interpretation yet for any statements about values of *P*-numbers other than 1 and 0. How then can we derive any consequence which is testable, even in a loose sense, from the hypothesis that the original *P*-number is $\frac{1}{3}$? So far every hypothesis about the number except that it is equal to 1 or 0 seems to be without relation of any kind to observable facts. I do not wish, of course, to suggest that hypotheses about a probability in the technical sense, other than suggestions that it is equal to 1 or 0, can ever be conclusively refuted; but I think it should be possible to explain more fully how we derive testable consequences from the hypotheses we make.

In the work to which Mr. Anscombe refers, Kolmogoroff has a section (chapter i, section 2) on the relation of his calculus to the world of experience. It would be useless to reproduce this verbatim without a rather lengthy explanation of his terminology,

but I think it can be summarized as follows : *Under certain conditions it is permissible to assume that a possible outcome of a certain kind of experiment has associated with it a P-number such that* (a) *we can be practically certain the frequency of this outcome in a large set of trials will not be much different from its P-number and* (b) *if the P-number is very small, we can be practically certain this outcome will not be found in a single trial.* Obviously these remarks about the use of *P*-numbers are of the sort we all want to make about the use of probabilities in the technical sense. But it must be remembered that they are not justified by any preceding interpretation for assertions about *P*-numbers. On the contrary, they are to be understood as *rules* from which assertions about *P*-numbers get such meaning as they ever have. Considered in this light, they are rather curious. ' Practically certain ' is an expression of common speech, and when we say in ordinary life that we can be practically certain of something we mean that it is very probable in the plain man's sense. (That the author intends us to understand it in this way may be seen from a note in which he says ' *Aus der praktischen Sicherheit zweier Behauptungen folgt die praktische Sicherheit der Behauptung ihrer gleichzeitigen Richtigkeit, obwohl der Sicherheitsgrad sich dabei ein wenig erniedrigt*'. Obviously the expression ' *man kann praktisch sicher sein* ' is used in the rules as an equivalent for ' *es ist praktisch sicher* '.) It seems then that we are only entitled to take Kolmogoroff's *P*-numbers for probabilities in the technical sense because he has laid down rules of application to connect them with talk about very high probability in the popular sense. And the question whether his theory is satisfactory as a whole can be decided only by further consideration of the popular sense of probability. So far as I know, his mathematics are not disputed ; but it is by no means obvious that his rules of application provide all that is needed for the removing of philosophical perplexities. Is our usage of ' we can be practically certain ' in fact such that we may properly make the claim in the circumstances he indicates without committing ourselves to more than his theory requires ? This seems to be the crucial question, and I do not see how it can be settled without inquiries of a kind Mr. Anscombe prefers to leave on one side.

(4) *Statistical Methods and Induction.* I hope no one has read section 41 of my book in the belief that he would learn there the latest developments of statistical method. For it was certainly not my wish to convey such an impression. My imaginary example about blood-groups on a South Sea island was intended only to show that an attempt to use the inversion formula of

Bayes might lead to great difficulties even when the problem was one about proportions in a finite population; and it was supposed to involve a situation in which an investigator tried to make the best of information not acquired by a pre-designed sampling procedure. I am very ready to believe that even in such a situation a modern statistician could do better than I had supposed possible. But I do not think this affects my main argument. In the section concerned I tried to make two points: (i) that the ampliative induction by which we establish probability rules of a kind analogous to natural laws is not the same as reasoning within the calculus of chances about the proportions to be found in a finite population, though both start from consideration of samples and both may use statistical devices; and (ii) that the science of statistics is concerned not with the theory of induction but with the analysis and presentation of data in such a way as to bring out collective features which may be relevant to inferences (whether inductive or of some other kind) from those data. Mr. Anscombe does not comment on the first of these points, but he agrees with what I say about the meaning of 'statistics', and I am therefore puzzled by his statement that modern methods facilitate the making of inferences in 'a more radical and rational way' than I suggest.

The deliberate practice of ampliative induction for the establishment of probability rules analogous to natural laws involves the *adoption* of some criterion for distinguishing between acceptable and unacceptable hypotheses, and I have suggested that the principle we follow is that of rejecting hypotheses according to which the facts we find would be the realization of a very improbable conjunction of circumstances. If the actual frequency of β things among the α things in our experience has been *f and our other information (in particular our knowledge of their distribution) gives us reason to believe that we may speak simply of the probability of an* α *thing's being* β, we assume that this probability is *about f*. (In my book the qualifications which I have now italicized were omitted or insufficiently emphasized with the result that my treatment of the topic was drastically over-simplified, quite apart from the error to which Professor Broad has drawn attention.) This policy is like one use of R. A. Fisher's Method of Maximum Likelihood; but I do not wish to suggest that his method is the only possible method of estimation to be used in connexion with any problem on which statisticians are asked to exercise their skill, nor yet that it is the best in all possible circumstances. Mr. Anscombe has given references to papers which show that the method may sometimes

lead to unsatisfactory results in the estimation of 'true values' for constants from a number of different measurements. It is not at all easy to characterize the reasoning involved in any attempt to get a true value from a number of different measurements, and I have nothing to say about that question. But I do not think these considerations have any bearing on the thesis which interests me. My contention is simply that any alternative to the policy I have described would be self-frustrating in the circumstances I have indicated.

Let us suppose, for example, that we choose instead the policy of assuming always that $P(\alpha, \beta) = \sqrt{f}$ approximately. Then if we observe a thousand α things and find $\frac{1}{4}$ of them to be β, we shall assume that $P(\alpha, \beta) = \frac{1}{2}$ approximately. But this assumption involves the consequence that it is very probable indeed that when we have observed a million α things we shall find about $\frac{1}{2}$ of them to be β; and if that proportion is realized in fact, our general policy will then require us to reject the hypothesis with which we started in favour of the hypothesis that $P(\alpha, \beta) = \sqrt{\frac{1}{2}}$ approximately. In short, the policy which we do in fact adopt in the circumstances I have indicated is the only one which allows us to hold it for probable in the sense of the theory of chances than an hypothesis of probability which it sanctions at present will be confirmed by future experience in accordance with its own criterion of confirmation. This is not to say that the policy enables us to assign to each hypothesis a probability in the sense of the theory of chances (for I still think that induction cannot be justified within that theory); but it is nevertheless a considerable recommendation. Our purpose in the deliberate practice of primary induction is to prepare the way for making predictions, and it would surely be foolish to go about this on a plan which committed us to saying that we should probably have to abandon most of the predictions we made on the strength of our first application of the plan.

Mr. Anscombe objects to the suggestion that the inductive policy, as I have described it, involves a special boldness. And he is right, if he means that an alternative such as the fantastic policy we have just considered would be bold in the same sense. For under the terms of either policy an hypothesis is open to refutation by any considerable change of relative frequency after the moment it is made. But I think he is wrong when he suggests that the boldness of which I spoke consists simply in the use of point estimates and might be eliminated by the use of intervals. It is naturally safer to use intervals—and safest of all to use the interval from 0 to 1, *i.e.* to give up making

hypotheses. If, however, a man made point estimates of probability ' by inspiration ', *i.e.* without adopting any general procedure for the selection of hypotheses, he would run no intellectual risk whatever. For it is characteristic of the situation in which we try to establish probability rules (as distinct from laws) that hypotheses cannot be tested in any way except by a criterion which belongs to a *policy* for selecting hypotheses. The really important commitment we make here in adopting the inductive policy is that of submitting our hypotheses to test by comparison with observed frequencies, and the running of this risk is essential to the pursuit of science.

Oxford University.

V.—THE DREARINESS OF AESTHETICS

By J. A. Passmore

British philosophers, with some few exceptions, pay little attention to aesthetics; it does not figure largely in Mind, nor is it considered a disgrace to a philosophy department when aesthetics forms no part of its curriculum. Can this lack of interest in a field so intensively cultivated in France, in Germany, in Italy, in the Americas, be set down, merely, to Philistinism? That is a conclusion not without its attractiveness; it is significant that aesthetics, when it is discussed, is so often condescended to as a poor relation of ethics. Discussions of ethics may begin as follows: "Is it true that moral distinctions are matters of taste, as is so obviously the case with aesthetic distinctions?" There is here displayed a willingness, almost an eagerness, to accept the view that aesthetic judgments are subjective (and hence, by implication, not worth discussing, suitable only for proclamation) which contrasts oddly with the concern that morals should have some more secure foundation. Yet we might well think that the evidence told all the other way, that one knows exactly how to draw attention to the weaknesses of a cheap novelette but is puzzled how to explain exactly why sentimentality is morally evil, that one can learn to distinguish the good from the bad in Indian sculpture or Chinese poetry far more readily than one can detect the peculiar virtues of Indian or Chinese modes of life. At least, we must be struck by the casualness with which aesthetics is so often dismissed; and there is certainly behind that casualness something of the Philistine conviction that Art, decidedly in contrast with Morality, is a thing of no particular importance.

Yet Philistinism is not a sufficient explanation of the prevailing distaste for aesthetics; there are philosophers who take works of art seriously and yet who share this distaste. Nor is this attitude by any means incomprehensible, for if books on aesthetics do not quite take the prize for dreariness, at least they stand very high on the list. Not unnaturally, a certain suspicion comes to attach to the whole subject; can it be that there is something about the nature of aesthetics which makes it peculiarly unilluminating? This Mr. John Wisdom[1] has recently

[1] In his contribution to the symposium *Things and Persons* (Ar. Soc. Supp. Vol. XXII).

argued, and it will be convenient to begin by considering the force of his denunciation.

He distinguishes between dull and interesting ways of talking about art. First for dullness—"One may pick up a book on art and it be very dull. It is dull when it tries to give rules, canons, which will enable us to deduce whether a picture or a poem is good. It is dull when it tries to set out in general terms what makes a good picture good" (4.1). How can it be more interesting? Perhaps by telling stories about artists—"how one was very poor and kept a little dog and so on". But this, as Mr. Wisdom rightly insists, is certainly not aesthetics; it is not even criticism. Then some people will find it illuminating if we talk about "what beauty is, what sort of difference there is between two people when one praises a poem and the other says it's very poor". But that, so Mr. Wisdom argues, is "meta-aesthetics", not aesthetics.

This latter point puzzles me. In the first place, Mr. Wisdom's apposition "what beauty is, what sort of difference there is between two people . . ." makes a very large assumption, the assumption that talk about beauty is really talk about differences between people; this might be a conclusion to which "meta-aesthetics" finally makes its way, but could scarcely serve as a general description of "meta-aesthetics". Secondly, the "talk about people" wouldn't itself be "meta-aesthetics"; it would simply be psychology. And finally, it seems very odd to call a discussion of "what beauty is" meta-aesthetics; that would rather consist, one might imagine, in trying to describe what aesthetics is, in the manner of this article. "What beauty is" is normally supposed to be the main theme of aesthetics itself—to be identical indeed with that general discussion of "what makes a good picture good" which Mr. Wisdom has already dismissed to the Kingdom of Dullness. The fact remains, and this is what, I think, Mr. Wisdom means, that talk about aesthetics isn't itself aesthetics, any more than "the philosophy of history" is history, and it could be interesting even when the conclusion which it sets out to demonstrate is that aesthetics itself is irretrievably dreary.

Thirdly, says Mr. Wisdom, a book on art can be interesting when the critic "helps one to a juster apprehension of the works he writes about"; in more detail, "a good critic by his art brings out features of the art he writes about, or better, brings home the character of what he writes about, in such a way that one can feel and see, see and feel, that character much better than one did before" (4.2). Here then is the other side in

Mr. Wisdom's antithesis; a dull book "tries to set out in general terms what makes a good picture good"; an interesting one helps us to see more clearly the character of a particular work of art.

Two points now suggest themselves: the first, that Mr. Wisdom doesn't mind explaining in some detail what makes a good *criticism* good, and he presumably doesn't think that his remarks are unilluminating. Now, I suppose it could be the case that writing about what good works of art are like is unilluminating, whereas writing about what good criticism is like is illuminating; but the curious thing is that the sort of remark which Mr. Wisdom makes about good criticism is precisely the sort of remark which many of us would wish to make about good literature in general. Good literature, I should be quite happy to say, "brings home the character of what (the author) writes about, in such a way that one can see and feel, feel and see, that character much better than one did before"; again, I should say that it is "revealing, moving talk" which is "not directed towards showing that a work (its subject) is good or bad" but "towards showing it to us for what it is"—other properties, these, which Mr. Wisdom ascribes to good criticism. To put the matter differently, what Mr. Wisdom calls a good criticism seems to me to possess qualities which overlap with those possessed by a good work of literature. And it is not at all plausible to argue that the very same remarks which are so illuminating when applied to criticism cease to give off any light when they are applied to literature itself.

The second point is this: when we call aesthetics "dull", we may mean either that it doesn't interest us or (what I take to be Mr. Wisdom's as it is certainly my own intention) that it fails to reveal with any sharpness the characteristics of its subject-matter. Now, it may just happen that Mr. Wisdom thinks that aesthetics is dull because he isn't interested in the general properties of works of art, but only in the character of particular works; he may wrongly believe that aesthetics is dull, in the only sense in which its dullness would be a weakness in it, merely because it doesn't do the kind of things he wishes to be done. *The Republic*, of course, discusses this very point: Socrates distinguishes between "the lovers of sights and sounds" and "true philosophers"; the former he says "are incapable of seeing or admiring the nature of real beauty" (V.246). Socrates is scornful of "the lovers of sights and sounds", but we all fall into this class as far as certain of our interests are concerned; we like travelling but abhor geography; contemplate

flowers with pleasure but know nothing of botany; take a lively interest in the lives of our neighbours but none in Freud. And so we find the travel-book illuminating but the geography text dull; enjoy a country-walk with the man who can point out flowers to us, without being won over to an admiration for Linnæus; are amused by the expert gossip but bored by the psychologist. So we shouldn't be surprised that most people prefer literary criticism to aesthetics, or that Mr. Wisdom should turn out to agree with them. This will not show that aesthetics is "dull", in the important sense of that word.

In the end, indeed, Mr. Wisdom somewhat relaxes his original severity. He grants that books on aesthetics may contain remarks "which are general and still not valueless". This happens "when some value is temporarily undervalued or perhaps has always been undervalued". And he gives an example: "Clive Bell emphasized the importance of the formal features of a work of art by saying that beauty depends entirely upon these" (5).

This admission is certainly the thin edge of the wedge. For how does Mr. Wisdom *know* that "formal features" have been undervalued? Surely, that he can know this implies that he has a general theory of "values" and their *real* importance, so that he can be led to reflect: "I had always thought that the subject was the valuable thing (what makes a good picture good), but now Clive Bell has led me to see that formal features are also important". Of course, his general theory isn't, in a way, of any interest to him once he has it; he will find books dull which merely repeat what he knows already; the only interesting general remarks, to him, will be those which challenge in some way the views that he has so far held. But this isn't a vicious sort of dullness; just as it isn't an objection to elementary economics that an economist may be bored by it. If new general statements about art can be illuminating, then so can old ones be.[1]

Mr. Wisdom doesn't then succeed, I think, in consistently maintaining that books which make general remarks about art *must* be dull, in a vicious sense; at the same time, I am prepared to agree with him that most of them *are* dull; why this dreariness,

[1] Mr. Wisdom's argument reminds us of a passage in Peacock's *Headlong Hall*:

"Mr. Gale: I distinguish the picturesque and the beautiful, and I add to them, in the laying out of grounds, a third and distinct character, which I call *unexpectedness*.

"Mr. Milestone: Pray sir, by what name do you distinguish this character, when a person walks about the grounds for a second time?"

if it is not inevitable? One good reason is suggested by Mr. Wisdom: "When it comes to what makes a good picture good or a good poem good the whole plan is a failure and is apt to lead not to understanding and discriminating feeling for what is good, but to that rigid and dead reaction to recognized points sometimes found in dog fanciers and characteristic of the Pharisees" (4.1). Books on artistic "goodness", to put the matter differently, often try to provide a substitute for individual judgment; they are dull for the same reason that books on scientific method are often dull; they set out to destroy spontaneity and speculation, and to substitute for them the application of a mechanical method. As Mr. Wisdom suggests, what misleads is that there are (more or less) mechanical methods for testing the goodness of arguments (although, even then, if we surrender our judgment to the rules we occasionally do quite silly things, as every teacher of logic knows). The aesthetician looks for a parallel method for testing the goodness of works of art ("follow my prescriptions and you *can't* go wrong"); the ethical theorist for a way of testing the goodness of conduct; the methodologist for a way of testing the goodness of beliefs. And this leads in every case to an emphasis upon "externals", upon techniques or whatever else lends itself to mechanical calculation. Thus the school-child has to write an "appreciation" of a poem; he looks up his guide to literature; there he finds some pleasantly definite statements about rhythm and rhyme and assonance and alliteration ("How skilfully the poet repeats the letter "p" to represent the patter of the child's feet!"). Rules about the "unities", rules about the couplet, about colour, about decoration or about dominants—this is the sort of thing a great many people are looking for and the theorist often sets out to supply this particular market. The things that matter about works of art can't be set out so simply; and even if we were to discover certain general features of good works of art, it would probably be quite as hard to tell whether a particular work has these features as it would be to settle on the general principles themselves.

One distinction must here be insisted upon: that between "technical" points and "formal" points. Technical points can be settled rapidly, by the application of rules; a formal point can only be made with difficulty, after the closest scrutiny of the specific work of art. To take an example from literature: the question—is this a sonnet?—can be settled (with some marginal doubts) by counting lines and rhyme-patterns. And, of course, nothing follows, from the fact that it is a sonnet, about

the aesthetic qualities of the poem; although conclusions can be drawn about the cleverness of the author. But the Aristotelian question (understood as Aristotle understood it)—has this work a beginning, a middle, and an end?—cannot be settled by any mechanical method; and this, the formal problem, is, I should say, an *aesthetic* question. There is, in the recognition of formal qualities, no "rigid and dead reaction to recognized points": to discover the form involves a strenuous effort, an exploration of a work which is always sufficiently novel to make an aesthetic consideration something of an adventure.

A main source of confusion (and consequent dullness) in aesthetics arises out of the failure to make this contrast between the formal and the technical, or to make it at the right point. Thus L. A. Reid in his *Study of Aesthetics* describes "technical" criticism in the following terms:—"It is the kind of criticism which, applied to painting, includes comment on composition, on the balancing of lines, planes and volumes, on tactual and colour values, on the expression of depth. In poetry this class of criticism is concerned with things like the disposition of vowel—and consonant—sounds or of assonance and rhyme and rhythm and metre. Music also has its own technical jargon" (p. 22). Formal and technical questions are here confused; what corresponds in painting to the discussion of metre in poetry (admittedly a technical question) is not the *formal* question—how the volumes in the painting are related—but rather the discussion of perspective or of brushwork, of the technical methods, that is, by which a painter displays to us a three-dimensional structure through the medium of a two-dimensional surface, a task which corresponds to that of the writer in presenting human feelings to us through the medium of words. Similarly, the "jargon" of the musician is in part technical and in part a method of describing the formal structure of the music (the disentangling of themes, and the description of their vicissitudes). It is not always easy to decide whether a particular question is technical or formal; some traditional problems (*e.g.*, the nature of style) may turn out to break down into a technical and formal component.[1] But certainly if all formal questions are dismissed to the limbo of "technique", aesthetics is left bombinating in a vacuum.

Thus there arises a sort of dullness which lies at the opposite extreme from marks-giving; it consists in saying nothing at all in the most pretentious possible way. "In clouded majesty here

[1] For a discussion of similar problems in music see D. F. Tovey, *The Integrity of Music*.

Dullness shone", as Pope put it. Or we may quote T. E. Hulme: "A reviewer last week spoke of poetry as the means by which the soul soars to higher regions, and as a means of expression by which it became merged into a higher kind of reality. Well, that is the kind of statement I utterly detest. I want to speak of verse in a plain way as I would of pigs—that is the only honest way."[1] To which, of course, one can expect the retort—"but poetry isn't pigs". No more it is: and there are special reasons why it is easy to make the slide from poetry to metaphysics, and not so easy to found one's metaphysics on a pig. For one thing, the ontological status of a work of art may provoke the typical metaphysical qualms; for another thing, poetry is often *about* what one might call, for short, metaphysical feelings.[2] But, still, poetry is a particular kind of thing, with its own properties, and it is worth (like pigs) taking seriously for itself. Aesthetics fails to illuminate, often enough, because the aesthetician wants to retain "mystery", rather than to dispel it, to conceal his subject rather than to reveal it. He wants to treat art instrumentally, as a "clue to reality"; his aesthetics is a spring-board to transcendental metaphysics. Consider, as illustration, the work of Jacques Maritain: "The music of Lourié is an ontological music; in the Kierkegaardian style, one would also say 'existential'. It is born in the singular roots of being, the nearest possible juncture of the soul and the spirit, spoken of by St. Paul" or again, "Why should a musical work ever finish? . . . Let us say that as the time of the world shall one day emerge into an instant of eternity, so music should cease only by emerging into a silence of another order, filled with a substantial voice, where the soul for a moment tastes that time no longer is."[3] Oh, for the *Journal of Agriculture* on pigs!

[1] T. E. Hulme, "A Lecture in Modern Poetry," printed in Michael Roberts, *T. E. Hulme*, Appendix II.

[2] For a recent, and particularly well worked-out, example of the transition from poetry to metaphysics see Professor H. D. Lewis on "Revelation and Art" (*Proc. Ar. Soc.*, Supp. Vol. XXIII), and for qualms about the status of works of art see Dr. Margaret Macdonald on "What are the Distinctive Features of Arguments used in Art Criticism?" in the same volume.

[3] Maritain, *Art and Poetry*, pp. 97 and 83. Admittedly the translation of this work is in places incredibly bad. But it is interesting to note that reviewers took phrases like this description of Stravinsky's music—"shaken by tentatives, sometimes astonishing, of galvanization"—in their stride; so used are they to reading nonsense in books on aesthetics that even when the translator piled nonsense on nonsense they were unperturbed, to judge at least by their silence about the quality of the translation.

Woolliness of this sort seems to have a natural habitat in certain fields: in education, in sociology, in metaphysics, as well as in aesthetics. Why should these particular fields be thus distinguished by so fine an array of empty formulae? One reason, certainly, is that the metaphysician, the sociologist, the educationalist, attempt to reconcile oppositions in a formula so generally applicable, so empty of any intrinsic content, that everyone can interpret it to suit himself. Thus the sociologist engages in dull and pretentious talk about society's being "an organic whole" when he wants to conceal the fact of social conflict; the educationalist talks about "the ultimate welfare of the child and of society" as "the aim of education" when he wants to make it appear that everybody expects the same kind of consequence from educational processes; the metaphysician, similarly, talks about "cosmic purpose" or "ultimate unity" in an effort to maintain that there is a single direction or pattern in the behaviour of things, a single end or purpose towards which everything works, in opposition to what seems to be the clear fact of the matter, that things work in very different ways, and move in very different directions.

The woolliness of education, of sociology, of metaphysics, is understandable, then, as arising out of the attempt to impose a spurious unity on things, the spuriousness being reflected in the emptiness of the formulae in which that unity is described. We can easily understand the passions which lie behind this anxiety to reconcile. But why should the same sort of thing happen in aesthetics?

There are problems here which demand more attention than theorists have so far devoted to them; but the facts are sufficiently clear—whatever the reason, people do in fact become tremendously excited about aesthetic differences. Non-representational painting, for example, arouses in many people an extraordinary pitch of fury, quite comparable to the horror with which they contemplate atheism or communism; they display a desperate anxiety to maintain that what they like is aesthetically good (and, more passionately, that what they dislike is aesthetically bad), objectively good, that is, for they are not all inclined to the view that "good" *means* "liked by me". Yet what they like is in fact such a hotchpotch of this and that, it being disreputable to condemn either Rembrandt or Sir Henry Leighton, or to praise either Cézanne or Salvatore Dali, that nothing but a high-sounding, empty formula can reconcile the irregularities of their aesthetic judgments. And the same applies, *mutatis mutandis*, to those who are affected by the desire to praise

whatever is "advanced"; sometimes, indeed, they share formulae ("sincerity", for example) with the bourgeoisie, for, as we said, the whole virtue of these formulae lies in the fact that they are infinitely accommodating.

The serious student of the arts is in a rather different position: he will be prepared to admit that he enjoys detective stories without at all granting it as a conclusion that they must have aesthetic qualities; or he may say "I can't bear to read war-books" without meaning by this that they are aesthetically bad. He may hang a painting on a wall because it is a painting by a friend, or of a friend, or because it reminds him of some familiar scene, while at the same time considering it to be of no importance as a work of art. It may still be true that he has not sufficiently distinguished within what he admires.

Let us consider the case of the literary critic. He will no doubt have realized that he sometimes likes what is trivial; he will not attempt to find properties peculiar to those literary works which he happens to like. But he may still try to find properties peculiar to whatever he *admires*, say to *Alice in Wonderland*, *Crime and Punishment* and *The Decline and Fall of the Roman Empire*. And then, I should say, he is forced either into woolliness, or into subjectivism (sometimes open, sometimes disguised), or into an emphasis on technicalities. For the properties to which we refer in describing the goodness of *Crime and Punishment* are conspicuously absent from *Alice in Wonderland;* and absent also (even if not so conspicuously) from *The Decline and Fall*.

No doubt there are resemblances between the three works; all three are written in words; all three have something to say about human beings. But still we judge them in quite different terms. It would not do for one of Dostoievsky's characters to disappear and leave only his scowl behind, nor for the Queen of Hearts to elaborate her motives; precisely what matters, to Dostoievsky, about Raskolnikov's crime is what would not matter, supposing Raskolnikov to be a figure of historical importance, to Gibbon.

Cannot we say that "a good style", at least, is a general criterion? Only if that word suffers most peculiar mutations in meaning ("style is the choice and arrangement of words", "the thought, that is the style too", "the style, that is the man"), since, in any ordinary sense of the word, Dostoievsky has a bad style, and many inferior novelists a good one. The meanings arrived at by mutation, however, are so vague that "style" no longer serves to discriminate at all. And if we are

content to say of all three that they are "works of genius" or "the precious life-blood of a master-spirit", we are involved in a manifest circularity. ("The work is great because the man is great." "What is great about the man?" "Why, his work!"); and we do not throw any light on the peculiar features of great works of literature.

What is needed, I suggest, is ruthlessness in making distinctions, a ruthlessness which will seem arbitrary only because the words we have to employ—"literature", "good"—have an honorific sense. Thus my saying of *The Decline and Fall* that it is "not literature" may be interpreted as a condemnation of that work, which is far from my intent. The works of Ethel M. Dell are literature; the works of Gibbon are not. Accurately to distinguish between literature and other "works in words" may be difficult (roughly, the literary work can be distinguished by its concern with human feelings as revealed in an incident); but it is sufficiently clear that we draw attention, in criticizing Miss Dell, to the absence in her work of properties we find in Shakespeare, not to the absence of the properties we detect in Newton, in Hume or in Gibbon.

So far, nothing very revolutionary has been said. It amounts to this: if we define literature in a fashion as wide as that presumed in the ordinary "histories of literature" we shall discover no distinctive properties (properties peculiar to "good works of literature") in the works we take to be good. One can find confirmation of this view in the actual practice of "historians of literature", as has recently been pointed out by René Wellek and Austin Warner in their *Theory of Literature:* "In practice we get perfunctory and unexpert accounts of these authors, historians, etc., in terms of their speciality. Quite rightly, Hume cannot be judged except as a philosopher, Gibbon except as a historian . . ." (p. 10). When it comes to the point the critic is unable (except for some very general remarks about style) to talk about Gibbon in the terms he uses to discuss Shakespeare.

But even those who will grant that it is not merely arbitrary to deny that *The Decline and Fall* is literature, may boggle at my second contention: that *Alice in Wonderland* is not *good* literature. Once again, this should not be read as condemnation. *Alice* is charming, amusing, a better companion than Dostoievsky on a desert island, a work of genius, etc., etc. But *Alice* has not the properties possessed by *King Lear, The Brothers Karamazov, The Love Song of J. Alfred Prufrock, Ulysses,* the properties in terms of which we estimate the work of Mauriac, of Graham Greene, of Maxwell Anderson, of Jane Austen, of Priestley, and

of Ethel M. Dell, properties regularly referred to by the ordinary critic of films, poetry, drama, and novel, properties which have clearly been in the mind of those literary theorists, from Aristotle on, who have not been content to lose themselves in mystification. If such properties are discoverable, and I think they are, it will not be merely arbitrary to describe as "good literature" the works which possess these properties. There is no reason why we should not continue to admire works which we yet describe as "bad literature"; the fact that they are not "good literature" does not prevent them from being "good thrillers", "good ghost stories", "good nonsense".

Then when shall we want to argue? Are we simply proposing a different and highly eccentric way of using words? Very much more, for what we have said can be contested at a number of points, and not on questions of usage. It can be disputed whether, say, *J. Alfred Prufrock* has distinctive properties in common with the rest of the group and not possessed by works we should admit to be bad (the ordinary novelette); or, assuming the group suitable, it can be argued whether, say, *Murder in the Cathedral* or Mauriac's *Thérèse* possesses the properties in question. And this, I suggest, is exactly how we proceed in the everyday business of critical discussion. Admittedly, we do not usually formulate an explicit list of good books; but, still, if we pick up a book, and read it in the expectation of its being good literature, we judge its claims out of our experience of Shakespeare, the great novelists and great poets; and if we depart from common judgments about what is great (rejecting, say, *Wuthering Heights*) this will be because we take the work in question to be of the character of a novelette, and so not to possess the properties we believe we can detect in good literature.[1]

Of course, there is no novelty in the attempt to discriminate within "literary works" between books which are to be judged in quite separate ways; and Mr. Carritt[2] has recently suggested that the attempt to make distinctions of this sort "leads nowhere,

[1] A method of the sort I suggest has recently been employed by F. R. Leavis in his *The Great Tradition*. It is possible to dispute Mr. Leavis's thesis by maintaining, for example, that works to which he ascribes certain properties do not in fact possess these properties; or, again, by arguing that the properties to which he draws attention would make it impossible for him to distinguish good literature from good moral theory (I am not saying that these criticisms could be made out, but at least they would be relevant). Merely to accuse him of "narrowness", or (as one particularly fatuous critic did) of ignoring the fact that we often read novels merely in the expectation of entertainment, is quite to miss the intent of his work.

[2] *A Calendar of British Taste*, Introd., p. xi.

although it has said many good things on the way". I am arguing, on the contrary, that it alone can lead anywhere; and I should quote, as an example of the impasse at which we arrive if we try to avoid such distinctions, Mr. Carritt's own aesthetics, summed up in the phrases "beauty lies in seeming, feeling makes it so"—conclusions, it seems to me, which are not aesthetics, but the rejection of aesthetics.

This is a remark which perhaps needs some elaboration, although it is not my present intention to discuss Mr. Carritt's work in any detail. Mr. Carritt lays down in the first sentence of his *Introduction to Aesthetics* what we might at first take to be a mere truism: "The subject of aesthetic philosophy . . . is obviously aesthetic experience". And he continues, "Its presupposition, therefore, is that rational sensitive beings such as men have a set of experiences pretty clearly distinguishable from others". This is unexceptionable, if all Mr. Carritt means is that there can't be aesthetics unless we can make aesthetic distinctions; but not at all obvious if it means that there must be a set of entities called "aesthetic experiences". And this latter is Mr. Carritt's interpretation. It then appears that the method of aesthetics is introspection and that "to deny that an experience is aesthetic out of deference to an aesthetic theory, although careful introspection testifies to its aesthetic nature, would be to tamper with the data on which alone the theory could be founded" (p. 19). Here we meet once again the Cartesian assumption that there is an inner world of "experiences", about whose nature we cannot be mistaken, provided only that we are reasonably careful in examining them. And, conjoined with this, the further assumption that these experiences carry their own labels with them, come to us neatly ticketed as aesthetic experiences, moral experiences, religious experiences. The outcome, as usual, is scepticism; we know our own experiences so thoroughly because they are ours, but we have no possible way of knowing other people's experiences. "Even in extreme instances", writes Mr. Carritt, "it is seldom possible to be sure of another's experience" (27). This is to put the matter too mildly; if anyone cares to say "in the presence of that object, I have an aesthetic experience", we might say "take another introspection", but if he takes it, and is of the same opinion still, that is the end of the matter; for us to pit our judgment against his would be absurd; to draw attention to weaknesses in the work he is contemplating would be to attempt, quite pointlessly, to rob him of an aesthetic experience which is rightly his.

It is my view that unless we can discover properties in good works of art which are not present in bad ones, we are inevitably led into this, or some comparable, species of scepticism ; and, further, that if we try to include in our theory whatever works are for any reason accounted " good ", no such properties can be found. Just at what point distinctions ought to be made is, of course, the serious question ; the proof, I should say, that a distinction lies here rather than there (for example, at the point I have elsewhere suggested,[1] between dream-works and exposure-works) is that a particular line of fracture gives rise to interesting generalizations. If it is true, as I have been suggesting, that no interesting generalizations (no generalizations which show us something about the distinctive properties of good literature) arise out of the study of " literature " as the ordinary histories of literature define it ; and that, on the other hand, there are interesting properties in such works as *The Brothers Karamazov, Macbeth, Tartuffe, The Love Song of J. Alfred Prufrock, Ulysses* and other equally interesting but different properties in *Alice in Wonderland, Kubla Khan* and *The Snow Maiden* then that is the only case that can be made out (and the only case that needs to be made out) in support of the distinction which I have proposed.

To try to avoid such distinctions is like trying to find characteristics common to science and to astrology, and peculiar to the pair of them as compared with other human activities. There are mechanical resemblances—both the astrologer and the scientist make calculations, for example—but these resemblances haven't the same theoretical interest as those which link physics with chemistry, and astrology with palmistry. The astrologer calls us arbitrary if we generalize about science in such a way as to exclude astrology from its ambit ; and many people will be indignant if we talk about literature in such a way that *The Decline and Fall* or *Alice in Wonderland* do not have the properties we ascribe to " good " literature. But this indignation misses the point at issue : the possibility of discovering general and distinctive properties must determine our field.

Now, even if there are things which are worth saying about " literature " (in the sense in which we have defined it), and other things which are worth saying about music, about architecture and so on, it still does not follow that there is such a thing as aesthetics, in distinction from literary theory, musical theory

[1] " Psycho-Analysis and Aesthetics " (*Aust. Jnl. of Psych. and Phil.*, Sept. 1936).

etc.; it seems to me possible at least that the dullness of aesthetics arises from the attempt to construct a subject where there isn't one. The alternatives have commonly been posed as if we had to say either that there is aesthetics or else that "it's all a matter of personal preference", but perhaps the truth is that there is no aesthetics and yet there are principles of literary criticism, principles of musical criticism, etc. We have no real difficulty in saying what is wrong with a cheap novelette, or what is wrong with the Albert Memorial, and we can do both of these things without being dull: the dullness arises if we try to develop a general theory of "wrongness" in art.

But surely, it may be replied, we speak of any form of art as "beautiful" or "ugly"—must there not be general properties which these descriptions convey? I should suggest, on the contrary, that there is something suspect ("phony") about "beauty". Artists seem to get along quite well without it: it is the café-haunters, the preachers, the metaphysicians and the calendar-makers who talk of beauty. We wouldn't feel quite comfortable if we called the etchings of Goya or the engravings of Hogarth beautiful; nor would we naturally employ that word as a description of Joyce's *Ulysses* or of Moussorgsky's *Boris Goudonov*. "Beauty" is always nice, always soothing; it is what the bourgeoisie pays the artist for; it is truth as compared with facts, goodness as compared with spontaneous creative action. In more professional circles, it is the refuge of the metaphysician finding a home for art in his harmonious universe, attempting to subdue its ferocity, its revelations of deep-seated conflict, its uncompromising disinterestedness, by ascribing to it a "Beauty" somehow akin to goodness.

When Lessing in his *Laocöon* (Ch. 2) objects to the work of Pyreicus "who painted, with all the diligence of a Dutch artist, nothing but barbers' shops, filthy factories, donkeys, and cabbages, as if that kind of thing had so much charm in Nature and were so rarely seen", when he demands (Ch. 24) that painting should "confine herself solely to those visible objects which awaken agreeable sensations" or that "one should not force expression beyond the bounds of art, but rather subject it to the first law of art, the law of Beauty" (Ch. 2), he has usage on his side. "Why, when there is so much beauty in the world, does the artist want to paint ugly women, or the novelist depict sordid scenes?"—that is the regular complaint of the Philistine. "Beauty", in this sense, is clearly relative to social conventions and individual disgusts; we may attempt to purge the word for the purposes of aesthetics, but certainly we shall be cutting

across the grain of usage.[1] The subjectivists are probably right about beauty ; but no consequences follow of any importance for aesthetics.

At least, though, we must allow talk about *good* literature, *good* architecture ? " That's really good " is the typical remark of artists and of critics. And it is applied equally to literature and to music, to painting, and to architecture. There's another phrase, often substituted : " he's really brought that off " ; this, I suggest, is what goodness means—" bringing it off "—accomplishing his special task. And in literature this involves something quite different from what it involves in painting ; the link between the two sorts of " goodness " is no closer than that between a good painting and a good theory, or between a good piece of business and a good shot at tennis. It is good because it is well done, but there is no sort of " well-doing " peculiar to art, although there is a sort of well-doing peculiar to each specific art.[2]

The remarks of artists are, of course, often of technical rather than aesthetic interest ; " he's done a good job " may mean " he has solved a ticklish technical problem ". Unless we insist, again, on the difference between technical and formal considerations, we may be bewildered by the apparent diversity of judgments about " goodness " ; one painter, for example, is praised for the clarity of his colours, and another because the objects on his canvas shade almost imperceptibly into one another. These are not, however, contradictory aesthetic criteria. We are in either case admiring the skill of the painter, but the aesthetic question remains : the question, that is, of the formal relationships between the three-dimensional objects thus depicted. These relations may sometimes be best conveyed by clear lines, sometimes by fuzzy ones. Similarly, what is a bad style in one context is a good one in another. The turgid rhetoric in which King Claudius explains his conduct to the Danish court is, in that context, precisely right ; we condemn

[1] Miss Helen Knight in her *The Use of " Good " in Aesthetic Judgments* (an article which I came upon late in the composition of this article, but with which I should agree on a great many points) suggests that " on the whole we commend the works of man for their goodness, and the works of nature for their beauty ". Perhaps beauty has a stricter sense in the description of landscape than in any other context and could be reserved as a description of the " natural ". Even then, however, it would need to be purged of its usual connotations ; for beautiful landscape is not always " agreeable " or " nice ".

[2] Here, especially, I find that Miss Knight has anticipated my conclusions. But I should hope to avoid the fragmentation of criteria which she takes to be inevitable.

Lyly's characters because they speak euphuistically, whatever the occasion, but it would be absurd to object to Osric on the same ground.

We have here distinguished a technical from an aesthetic use of "good". How can this be done if there are no aesthetic properties? A good detective story need not be a good work of art; people who "like to curl up with a good book" aren't passing aesthetic judgments. It is therefore impossible to define "the aesthetic use of 'good'" as "the use of 'good' in which it is applied to works of art", for we can rightly describe a work as "good" without ascribing aesthetic characters to it. The solution, I think, is that although there are not "aesthetic properties" common to all good works of art, there is what we may call an aesthetic approach to works of art, just as there is a scientific way of considering a thing, without it being the case that things have scientific characters; or again, there are not technical properties but there is a technical approach. The technical approach raises the question: "how was this work put together?"; the historical approach: "when was it done, influenced by what?"; the biographical approach: "what does it amount to, as an event in its creator's life?"; the aesthetic approach: "how does this work hang together?". But "hanging together"—or, to use more genteel expressions, coherence, harmony, integrity, form—is not a special aesthetic property, recognizably the same in various works of art; it is more like a category than a property, suggesting the kinds of question which are to be asked rather than the sort of property that is to be looked for. We only arrive at the level of properties when we ask specific questions about specific works.

This doctrine, that we must seek different properties in different art-forms is not, of course, a new one; Croce attacks it in his *Essence of Aesthetic*.[1] But Croce very much confuses the issue by attacking two quite distinct "prejudices" at the same time; the first, the doctrine of literary kinds (tragedy, comedy, pastoral, epic, etc.), the second, the doctrine of aesthetic kinds (painting, music, literature, etc.). To the first one can certainly object, as Croce does, that the boundaries between, say, tragedy and comedy can never be fixed with any precision, but do we ever find ourselves in doubt whether we are confronted by a symphony or by a statue?

What Croce would have us believe is that whether we are contemplating architectural masses, or a Bach fugue, or a play by Shakespeare, or the paintings of Cézanne, what we are in

[1] As translated by Douglas Ainslie, p. 53.

every case *really* contemplating is a certain form of human feeling. This is a particular variety of aesthetics now widely accepted. Whether the feelings we then admire are supposed to be those of the artist, or whether we admire our own feelings anthropomorphically ascribed to inanimate objects—"we transform the inert masses of a building into so many limbs of a living body, a body experiencing inner strains which we transport back into ourselves"[1]—the suggestion is in either case the same, that nothing inanimate can have aesthetic properties in itself, that nothing is "beautiful" except the human spirit. Art then most eminently plays a metaphysical role: it stands for the animistic as against the scientific view of things, showing what things "mean" as distinct from what they are. Whatever describes the work itself is dismissed as "technical"; aesthetics is no longer particularly concerned with works of art.

Such a theory, however, can never show that a work "means" one thing rather than another; for what a work "means" will depend upon what it suggests to a particular observer. (And there is nothing here to correspond to the rules of linguistic usage; to call architecture a "language" in the hope of thus avoiding arbitrariness is to pretend there is a convention where none in fact exists; nor should we seek to establish one.) Mr. Carritt's scepticism is the only honest conclusion of all such doctrines. That the *Pastoral Symphony* is more fully expressive of cheerfulness than *Pop Goes the Weasel* or that Durham Cathedral is more expressive of religious feelings than the Methodist Chapel at Llangollen—these are matters regarding which argument would be absurd; it is as if, to take a strictly comparable case, one person were to argue that Monday is a yellow day and another that it is a red day. Concerning such merely personal associations there can certainly be no dispute. Meanwhile people go ahead with their ordinary aesthetic discussions, criticising literature as literature, music as music; asking what the work of art *is*, not what it "means", quite as if they were concerned here, as in science, with particular matters of fact. And if this were not so, criticism, education, controversy, would be impossible.

To return to our main theme, there is no way of *proving* that good works af art have no distinctive properties in common (properties, that is, which are not to be found also in bad works of art or in whatever is well done, whether a work of art or not).

[1] Lotze, *Microcosmos* (Bk. V, Ch. 2) as quoted in Vernon Lee's *Anthropomorphic Aesthetics* (in his *Beauty and Ugliness*, p. 18). See for a critical examination of "expressionism", John Anderson, *Some Questions in Aesthetics*.

One can only draw attention to what happens when aestheticians try to nominate such properties. Sometimes the aesthetician substitutes for aesthetics something quite different, for example, the theory of art. For certainly art (good and bad) may be considered as a social institution; we can ask how it arises, what social effects it has, under what conditions it flourishes; there is a phenomenon, art, which can be studied socially, psychologically, or ethically. Or else, and very commonly,he substitutes metaphysics for aesthetics; or, on the other side, the study of techniques. Sometimes the characteristics which the aesthetician mentions—unity, structure, integrity—are general categories rather than anything at all peculiar to works of art; here, we have suggested, the aesthetician may unwittingly be on the right track, but certainly he has mentioned no aesthetic properties. And sometimes (this also can be illuminating) he stretches modes of criticism which are applicable enough in a particular art in the attempt to make them cover every art. Thus, if it be said that good works of art "develop a theme", we know what this means in music, we may feel that we can make some sense of the phrase in criticizing literature (although already, I suggest, we really mean something quite different), but how could we possibly apply this critical method to sculpture? Similarly, "balance" may have a specific meaning in the plastic arts, but can it be non-metaphorically applied to literature and to music? And more often than not, the aesthetician does none of these things; he simply talks dreary and pretentious nonsense. The alternative, I suggest, isn't subjectivism but an intensive special study of the separate arts, carried out with no undue respect for anyone's "aesthetic experiences", but much respect for real differences between the works of art themselves. In this sense—art for art's sake!

University of Otago, N.Z.

VI.—THE ARGUMENT FROM ANALOGY AND OUR KNOWLEDGE OF OTHER MINDS

By J. F. Thomson

The commonsense answer to the notorious question "How do we know what another is feeling or how he sees the things that we see?" is, I suppose, that it depends on who the other is and who we are and what it is that we know. This will not satisfy one who asks this question and who is worried (or professes to be worried) about the possibility of knowledge about other minds; for what he demands is not commonsense but a justification for claims to such knowledge. This demand is, however, mistaken. For the situations in which we know about other minds are many and various and have nothing more in common than that they are situations of this kind. We must in fact distinguish between claiming that there are such situations and claiming to be in one. The former claim does not need to be defended; the latter may. The confusion results from laziness. Moore, by holding up his hands, reminded us that there are situations in which it is absurd to doubt something; in this case existence of a material object. Such situations can be described, in long and laborious detail; but this is hard work. Hence we rely on short schematised examples; we say "tears", "grief", we use proper names as though they were a sort of variable description, "Smith", "Jones", where Smith and Jones are really any persons you (anyone) cares to think of, and then are surprised that we feel doubts about these shadowy creatures of our imagination that we do not feel in real life. Hume contrasted the study and the market-place, but he never tried to bring the latter into the former. There is then no question of *defending* commonsense, but only, as so often has been said, of seeing as best we can what makes us puzzled about it. In what follows I want to try and indicate one possible source of doubt about other minds, a source which I think it has in common with a much older puzzle, that about free-will. Most of what I have to say can be presented as a discussion of the argument from analogy.

I

In discussion of the argument from analogy we sometimes compare it with an argument about a house; we compare the way we

might be said to argue from tears to grief or from winces to pain to the way we might infer from facts about the exterior of a house that a tea-party is going on inside. The point of the comparison is, of course, to discredit the suggested inference from tears to grief. The inference to a tea-party is permissible, for here we have a way of testing its validity; we have at least the chance of going into the house and seeing the party at first hand. This, however, suggests that we ought to be able to test the inference that a person is sorry about something in the same way, by inspecting his feelings at first hand; and we are not sure what, if anything, this means. From this we conclude that to argue "analogically" about other people's feelings is impermissible; hence, that we do not argue this way.

Now, whatever else may be said of it, the comparison with the house-inference is misleading in this respect at least; it suggests that the only sort of evidence we have for how a person feels is his behaviour, what he does. But we also have as evidence what is done to him. The evidence for grief lies just as much in grief-making situations as in tears. Doubtless, tears and screams affect us. But we know quite well that tears may be tears of joy, screams, screams of rage, that what look like winces are sometimes tics. And if we are really in doubt as to whether a man is in pain, we do not peer more closely at his winces; we look around to see if something is hurting him. Or if we think a friend is huffy with us, we may well ask ourselves what we may have done to give offence.

People do not (as far as I know) feel pain or become huffy without reason. To the extent that they do, we don't know very well what goes on in their minds. And the reason why they are in pain (or huffy) is often our reason for supposing them to be in pain (or huffy).

Suppose then we find a man in a dentist's chair having a tooth drilled. We may suppose him to feel occasional twinges and sympathise. The question whether we *know* that he is in pain must of course wait on further description of the situation. If, however, we seemed not to sympathise, a moderately plain stander-by might say:

You just haven't any fellow-feeling.

You are not putting yourself in the other man's place.

You've got to learn to see it from the other fellow's point of view.

You would not (or you do not) like it done to you.

These remarks suggest a reason for sympathising and therefore

also a reason for supposing the man to be feeling occasional twinges of pain. Whether these reasons are good ones depends of course on the facts of the case. Perhaps we did not intend to waste our sympathy on someone who was not in pain. Perhaps we had put ourself in the other fellow's place, seen it (the situation, his situation) from his point of view and found it by no means unbearable. Perhaps we think that what we do not like done to us is not being done to him, but that what is being done to him is something we do not mind being done to us. And perhaps we are right. But these reasons *may* be good ones ; which is to say that the stander-by may have sized up the situation better, more accurately, than we. Perhaps then we had not put ourselves in the other man's place, for we had not quite gathered *how* he was placed. . . . Do we need to be reminded, how, in the case of more subtle and complicated feelings than those of pain, a lack of tact and sensibility may prevent us from *knowing how a person feels*, from *seeing his point of view*, from *feeling ourself into his situation*, and so on, or how closely a person's tact and sensibility are connected with the sort and extent of his experience?

We suppose that when certain things are done to a man he feels pain, or that when certain things are said to a man he feels huffy. If we suppose that these things are being done to a man we shall probably suppose that man to be in pain. And in lots of such cases we shall be right.

This might well be called arguing by analogy, analogy with similar cases. We learn the meaning of the word "pain" (we learn what pain is, or what pain is like, or what it is like to be in pain) by being in dentists' chairs and such places. This process *is* a process whereby we learn what it is like for a man to be in pain—just because we are men who learn and suffer. One might equally well say that when an Englishman is in pain he learns what it is like for an Englishman to feel pain ; and the only reason we do not say this is that we do not know of any interesting connexion between feeling pain and being English. At the same time we learn what sorts of situations are painful; the world being as it is, it is difficult to learn what pain is like without also coming to have some idea of what causes it. So if someone, A, has learnt what it is like for a man to be in pain then *a fortiori* he already has some idea what it is like for a man, B, to be in pain. The extent of his idea will in general depend on the extent to which he supposes it *relevant* to describe B as a man. This again will depend on what he supposes B's situation to be.

To understand what is meant by saying that someone feels toothache, I do not have to have had toothache myself. If I have at some time had an ache in my head, and know where my teeth are, or where people's are in general, I can then understand what is meant by saying another has toothache, understanding by it that he feels sensations similar to those I had when I had a headache but located where I locate non-painful sensations in my teeth. It might be that if I had never felt pain myself I could yet understand what was meant by "He has got toothache", understanding by it that he does not like the situation he is in, that the situation in which he finds himself is partly similar to a situation in which I found myself and did not like, is *nasty*. Say, if you like, that this is not all that is meant by "He has got toothache"; it is not. But do not say that I should have *completely failed to understand*, because I should not. One of the most curious things about this puzzle is the way in which philosophers have failed to make the obvious distinction between knowing exactly how someone feels, knowing fairly well how he feels, knowing roughly how he feels, and so on.

If I do not know about dentists' chairs, I can be *shown*. I can be put in the relevant situation, so that I come to say: "Now I know what it is like, now I know how he felt". Whether this claim to know is acceptable will depend on how much the situation in which I was put is like the situation in which he was.

But somewhere around this point familiar doubts may arise, doubts which may find expression in some such way as this: "But can you ever be certain that one case is sufficiently like another to justify your inference? After all, though it can of course be shown to you what it is like for *you* to be in a dentist's chair, for *him* to be in a dentist's chair is something quite different, and you can never really know what this is like. For you can never be in another's situation or see yourself how things seem to him."

Does the sceptic warn us that some tasks are more difficult than we had supposed, remind us of mistakes we had not thought to guard against, or does he rather absolve us from making mistakes, *un-worry* us by showing us that the tasks we thought hard were impossible? To understand his scepticism (and our own) we have to see how his radical doubt about the very possibility of knowing about other minds *grows out of, is a crazy extension of*, practical commonsense doubts about particular inferences about how a particular other feels or thinks. Go back to the man in the chair. Commonsense objections to supposing him in pain might be:

"Perhaps the dentist is only drilling on his bridgework."

"This man has had a terrific amount of Novocaine, it would be very surprising if he really felt anything."

"This dentist is really very good, he hardly ever hurts."

"Perhaps this man is one of those curious people who cannot feel pain at all."

One who raises these points relies as we did on commonsense law-like statements ("anæsthetised people don't feel pain") and like us he uses them to get to a conclusion about how the man is feeling ("he is not in pain"). Secondly, he does not question the wisdom of in general relying on the law-like statement we relied on, he just suggests that it is not applicable here ("this is not a man having a tooth drilled"). Thirdly, he does not just say that we may be mistaken, he mentions the mistake we might have made, and thereby gives us a way of settling the question whether we did make this mistake. It is this above all things that makes his doubt a *practical* one. To see how brooding on the possibility of making mistakes may lead us, absurdly enough, to scepticism, let us see too how it may lead us to the puzzle about free-will.

What I have called "law-like statements" are like high-grade scientific laws in this respect, they are *about* classes but *apply* to the members of those classes. Statements about people in general are *about* people; they are not *about* John, Tom and Mary, though they *apply* to these. Similarly, the laws of planetary motion are about planets and not about Mercury and Neptune though since these are planets the laws apply to them. The difference between high-grade and low-grade is this. In making predictions about Mercury we apply all the laws we can, but these are fairly simple and few. Human beings are, however, more *complicated* than planets in that they respond in more complicated ways to more subtle and variegated features of their environments. Hence our low-grade statements admit of exceptions, and it is more difficult to be sure which ones—and we all know thousands—we want to apply. It is wrong here to say we never know all the initial conditions; wrong, because we sometimes do know all the initial conditions we need to know to make successful predictions, and it is difficult to see what else "all the initial conditions" could mean. What *is* true is that sometimes we do not know all the initial conditions we should like to know, and that often in such cases we do not know *what* initial conditions we should like to know that we do not know or even that there *are* others that we ought to know but do not. (We should like to know all the relevant ones; obviously.)

Will Smith, who is both a Catholic and a Liberal, vote Right or Left in the next elections ? If we can be said to know the answer to this, it is because we know Smith and know more about him than that he is a Catholic and a Liberal. That is, we have to have a fair understanding of people in general and be acquainted with Smith. It may be, though, that however well we know Smith we still cannot be said to know how he will vote. Of course it remains possible that increased sensibility and cleverness on our part might enable us to know. But then again they might not. So not only may it be that we do not know, it may be too that we cannot know, do not know whether we could know, and cannot know whether we could know. This situation we may well find intolerable, and we may seek to escape it by one of those wretched compromises so dear to philosophers. If we cannot predict or do not feel confident in predicting Smith's vote, it is either because we cannot get at certain facts about Smith, or because we are not certain what other facts to look for that we have not got, not even being certain that there are other facts we ought to have ; or, more likely, a mixture of these. It is of course the latter sort of ignorance that worries us. And we may be led to suppose that there are some *very special facts* about Smith, facts about Smith's *essential nature*, of which we are ignorant and possession of which alone would enable us to predict his behaviour ; putting, as it were, these *very special facts* in to replace the ordinary common-or-garden facts which might enable us to predict (we do not know whether they would or not). These special facts are then supposed to be related to how Smith will in fact vote in the same way as his being a Liberal might be related to his voting Left. The feeling we may have about this is neatly expressed by Hopkins :

> Man lives that list, that leaning in the will
> No wisdom can forecast by gauge or guess.
> The selfless self of self, most strange, most still,
> Fast-furled and all foredrawn to No or Yes.

" All foredrawn " ; it's already decided, in some sense, how Smith will vote ; decided in (and by ?) his inmost nature. But we can never, never, know which way it is decided ; perhaps (it is hinted) because there is really nothing to know. The innermost sanctum which we should have to invade is empty, selfless ; *i.e.* there is no such place. (And is it really Smith who decides ; does he not just live his list, behave Smithily ?)

To the extent we decide that the sanctum is empty we shall say grandly that in principle Smith's behaviour is fully predictable.

Something like this may be right, except that "fully" is the merest bluff (and so really is "in principle"); we have mortgaged the future of science for our own peace of mind. To the extent that we subscribe to the inner-sanctum theory we shall say grandly that in principle Smith's behaviour is not fully predictable—again, something of a bluff, or at least a whistling in the dark. Is it necessary to remark that we *just do not know* how much of a man's behaviour is predictable? (And it is not our business to find out either.)

A similar temptation operates in the case of "other minds".

II

The word "this" is not used to describe, not even to describe a class of proprietary what-nots called particulars, entities, or individuals; but to refer, and to refer to all kinds of things. Neither is "Smith" used to describe. (Hence knowing Smith is not to be equated with understanding the description "Smith"). "I", "you", "she", "he", "other", are also used to refer, usually, though not always to people. Of course we know this, but various considerations may tempt us to talk and think as though we did not.

Imagine a woman to say; "I want to be loved for myself alone". What is it that she wants? We do not know, unless we know her; and this is only an example. But the sort of answer that might be correct is seen in these lines of Yeats:

> I heard an old religious man
> But yesternight declare
> That he had found a text to prove
> That only God, my dear
> Could love you for yourself alone
> And not your yellow hair.

We see the difference between loving a woman for herself alone and loving her for her yellow hair, between wanting to be where she is (etc.) and wanting to be where her yellow hair is, even though these are the same place; for one who loves her for herself alone may want to be where she is when the yellow hair is silver; Love's not Time's fool, and so on. But do we see thereby what it is to love her for herself alone? We do not; because this is only an example and we are not talking about a real woman at all; but *not* because the "logic" of the phrase "for herself alone" is very mysterious.

Someone might say: "I love her because, well, because she's just Mary". In a sense he says why he loves her, in a sense he

does not and cannot because he does not know. *If* we know who Mary is, or, better, if we know Mary, it might be as though he had said "I love her because—well *look* at her—wouldn't you?" One might say then that he *shows* us why he loves her, by calling attention to certain facts about her, which he sees but cannot describe. But his remark can only do this job if his hearers know to whom he refers by using "Mary". If you heard this remark on the top of a bus you would not learn much from it, only that someone behind you loved someone whom he called "Mary" and had not much idea why.

We regard some facts about people as more important than others. Yellow hair turns silver, people alter, yet something about a person remains the same. Also, people (are said to) hide lovable personalities behind gruff exteriors and so on. These distinctions and others we are sometimes led to lump together and express as a distinction between what is internal and what is external, or between what is interior and what is exterior, or between what is essential to a person's make-up and what is (merely) accidental, or even, if we read the continental philosophers, between what is subjective and what is objective. (Since we often regard facts about how a person behaves as more important than facts about his or her appearance, this latter distinction is easily assimilated to the traditional Mind/Body one.) And because of this we do find uses, *sometimes* sensible ones, for phrases like "his inmost self", "the real Smith", "loving her for herself alone" and so on. But if we try to consider them apart from the social contexts in which they are used, we run the risk of committing something like a type fallacy. Thus, we may suppose that "loving her for her yellow hair" and "loving her for herself alone" name different reasons for loving her *of the same sort*, and, that *in the same way as she has yellow hair* she has a thing called a self. To assert that people have not got selves is, of course, to make the same mistake. These phrases are not then descriptions of occult entities or regions of maximum opacity in people. But the reason for saying this is not that people are completely transparent—they are not—or that they have not got 'real selves'—they have—but that these phrases are not descriptions.

It is perhaps by failing to notice this that people come to say things like:

> For it is the particular, the unique and concrete individual, which cannot be approa hed by rational methods, and not the abstract universal. Science can describe general types of landscape, for example, or of man, but it can never exhaust one

> single individual landscape or one single individual man . . . the unique individual and his unique actions and experiences and relations to others can never be fully rationalised.
>
> (K. R. Popper, *The Open Society*, Vol. II).

We should have to ask here what is to count as exhaustive. For of course, no one thing does. This passage exemplifies, I think, the mistake under discussion; that of supposing that we try *awfully hard* to describe queer things called individuals but never quite bring it off, because these are elusive and being unpredictable don't lend themselves to rational discussion. In the sense in which there are dogs and cats there are not also individuals which *dress up* as dogs or cats at will, putting on their fine conceptual raiment to seduce scientists but being unmasked by metaphysicians; in the same sense in which there are men, there are not also individual men who are somewhat more difficult to get on with. One might almost say that philosophers have *invented* individuals and particulars to be the things described by those words which do not describe at all.

There is, however, another, related yet different, way in which we get our notion of the self. Each of us uses "I" and uses it to refer to himself. Each of us uses "man" and could use it correctly to describe himself. Neglecting the difference, we may unconsciously look around for some property which all "I"-users have and the having which makes it correct for them to use "I"; and find this property in that of being a self. Doubtless we might rather find it in that of being human; we do not, partly because we have read books in which animals talk and use "I" correctly, partly because we are looking for a *queer* property and that of being human is drab in comparison.

This danger is increased for most of us by the prevalence of a curious use of "I" among philosophers whereby they use "I" not exactly to refer to themselves but almost as though to refer to their properties of being selves or knowing subjects or something of this sort. Thus, if someone asked "How do I know what another is thinking?" and I replied (or anyone replied) "I do not know you well enough to be able to say" the reply would be thought silly. But that it would is important. For it shows clearly that when a philosopher asks "How do I know . . . ?" he isn't using "I" in quite the same way as one who asks "How am I able to jump five-barred gates so easily?" (Although there are resemblances, as we see by considering "How do I breathe?", "How do I digest my food?" which come between these.) Thus compare "How do I know. . . ?" with "How does he know . . . ?" The latter may mean and

often does; "How did he come to know? "Who told him?". The former rarely means: "How did I come to know?" though it may (who was it told me that?). To answer the latter we usually have to know who is being referred to, but the former tends to start the same conversation whoever asks it.

At the risk of being tedious: Professor Ryle's section on Solipsism in *The Concept of Mind* contains twelve occurrences of "I" and three of "my" and "mine". "I can witness what your body does, but I cannot witness what your mind does. . . . In making sense of what you say, I am not inferring to the workings of your mind, I am following them. . . . I have no other way of finding out what you have been saying or picturing to yourself. . . ." But despite Ryle's assurance elsewhere that when Gilbert Ryle uses "I" he uses it to refer to Gilbert Ryle, one would hardly say that this section was even in part about Gilbert Ryle, that it was self-descriptive or self-revelatory, or about the author's special abilities and failings. It's not about Ryle but about Solipsism. He could have used "one". But then, one what? Reflecting that animals and God are said to know what is going on in other minds, we see that this section is properly about how anything knows that can be said to know. And it is tempting to say that it is thus about selves or knowing selves or knowing subjects. Clearly Professor Ryle intended his thesis to apply to himself; he presented his own case as an example of how people in general know. But just the point is that only descriptive words and phrases are said to apply. And by using "I" in this way we run the risk of supposing that for philosophical purposes we have to regard ourselves as, so to speak, *undressed*. (Compare the existentialist use of phrases like "naked subjectivity".)

It follows that if someone makes the classic challenge: "I can never really know . . . ?" we are entitled to sympathise, or to say "Then do something about it". This throws up a curious fact about the unofficial picture we sometimes have of the egocentric predicament. We imagine ourselves locked up in a soundproof cell, say. But by extending our plight to others we do in a sense admit there is no predicament at all. For if one were locked up in a soundproof cell, one just would not know about other people; they might all be chatting happily in one room next door. And if one could not know what was going on in other people's minds, could one be certain that they often did not know what was going on in each other's minds? The curious use of "I", to refer to oneself and all other "I"-users simultaneously, blurs this point and allows us to imagine ourselves at the

same time both incarcerated and looking down on all the cells from on top, making sure that the other prisoners don't avail themselves of that liberty which we ourselves pretend to have surrendered. At the same time, if we suppose having or being a self to be a fact about a man in the way that being deaf-and-dumb is a fact about him, we shall find this extension natural enough.

It is, of course, this same use or misuse of "I" that gives its peculiar flavour to solipsism. "Only I have sensations; other people merely behave." Compare this with "Only I really love her; the others just send her flowers (etc.)", or with "Only I really understand this proof; other people just know it by heart and recite it parrot-fashion", and then ask how each of the speakers would reply if asked (not to justify his claim but) to *explain* his unique position. It is pretty clear that even though the solipsist supposed himself to be (say) the only red-haired person in the world he would not explain his uniqueness by referring to this fact. He would like to say "Only I have sensations because only I am *me*".

But a complete understanding of the motives that go to make up solipsism and doubt about other minds require us to give up the view that "I am me" is a tautology, a truism, or analytic.

We have seen that one of the ways in which we get our notion of the self is one that should allow fully-fledged, completely-rarified selves to have only one property, that of being themselves. To say of anything that it is itself is to say something formally true irrespective of subject-matter, *i.e.* irrespective of what sort of thing we are talking of. Hence the formal property of self-identity is admirably suited to be a defining property of the class of entities, particulars, individuals or what-have-you. But we also have uses for "I am me", "Mary is Mary" and syntactically similar sentences which require to be kept distinct from that use of, say, "Mary is Mary" that would make it an interpretation into the domain of human beings of the open formula "$x = x$". It is these other uses that make the use of words like "analytic" dangerous.

Each of us feels himself to be different from all other men; and we sometimes seek to express this feeling by saying "I'm me". Our feeling about this is probably correct. For pretty obvious commonsense reasons, which have nothing in particular to do with time and space, human beings are unique, though of course more or less like each other in various respects. To say that a person, say Smith, is unique is to say that for any other person x you choose you can find some fact y about Smith such

that y does not hold about x. This is, of course, different from saying that there is some fact about Smith (*e.g.* his essential Smith-ness) which will serve to differentiate him from any other person you care to mention. These two formulations would be the same only if we were told beforehand all about the people who were possible candidates for the post of x. But we suppose ourselves to be different from people we have not met and from those whom we have met but do not know very well, not merely from those we know. In terms of quantification-theory the difference is that between

$$1.\ (z)\ (x)\ (\mathrm{E}y)\ \mathrm{F}zy. \supset \mathrm{F}xy,$$
$$2.\ (z)\ (\mathrm{E}y)\ (x)\ \mathrm{F}zy. \supset \mathrm{F}xy,$$

and it is easy to find domains in which one of these is satisfiable but not the other.

We should then find our uniqueness in ourselves, or if you like in the fact of our being ourselves. But this is not to say, in our being self-identical; for others are this too. We cannot say how Smith and another are (interestingly) different until we are told who the other is. But we are pretty sure that whoever the other is there will be a difference; and this hunch we express by saying that after all Smith is himself and the other man is *him*self. The suppressed variable y now bears the same relation to Smith as her kindness or generosity or some other reason for loving her bore to Mary in the example "I love her because she is Mary".

To ask "What makes Mary herself?" would normally be to ask for an explanation of certain characteristic modes of behaviour she adopts—the questioner assuming of course that we know Mary or know who she is and what ways of behaving are characteristic of her. The question is sensible although asking it of people who do not know Mary is not sensible. Again, one who says "Ah, be yourself!" is not enjoining us to have just those properties we do have and no others. He assumes we could be acting more naturally (or more pleasantly?) than we are and asks us to do so. His request is sensible if his assumption is correct, and this again depends on the situation in which the (token) request is made.

So "I am me" and "She is herself" have a trivial use and a non-trivial use and occasions of their non-trivial use may be sensible or not sensible. (This is not to say that a non-trivial use may not be more or less eccentric.) The possibility of confusion, between saying of something that whatever happens to be true of it happens to be true of it, and referring

to a thing and drawing someone's attention to what is true of it, between saying that Napoleon was self-identical and that Napoleon was after all Napoleon, is tremendously increased when we are considering people in general.

III

These complications are directly relevant to the problem of other minds. We tend to have the picture of a little man (the self, the Ego, the knowing subject) who sits inside the skull and looks out through the eyes ("the windows of the Soul"). And it is this picture which imposes on the argument from analogy the comparison with the argument about a house. We feel that what ought to correspond to entering a (real) house and making sure there is a tea-party is getting into someone else's mind and making sure there is pain. Now we should be able to see why we feel this and why (in a sense) we are right in feeling it. The trouble is that our picture of the self and of ourself is not clear. Certainly we are inhibited by the strangeness of what we find ourselves saying about observing sensations—or if we are not we should be. But this is not all that inhibits us. In so far as the little man is felt to be just an undressed particular—a "bare that" in Bradley's phrase—there should be no trouble, no trouble at all, about his moving house. Completely rarified selves could nip in and out of different bodies (*and* of different minds) twenty times a day and no-one be any the wiser or the world different. This Leibniz saw: "Of what use would it be to you, Sir, to become King of China on condition of forgetting all that you have been? Would it not be the same as if God, at the same time as he destroyed you, created a king in China?" Well, would it? To the extent we cannot quite persuade ourselves that it would, it is because our notion of the self is not that of a completely rarified thing. Our metaphysical notions are not consistent and thoroughgoing (they are metaphysical). Though we are encouraged to think of ourself as something tenuous, something of which no more can be said that it is there and that all other conscious beings have one too, we are also encouraged to think of it as that which is most characteristic of us. And this is what inhibits us most, I think, about the possibility of metaphysical house-removing. We are not sure how much of his furniture the little man would have to take with him to remain what he is, or how comfortable he would be with another style of interior decoration. And this doubt is due, not to our having been given by language the idea of something corresponding to the phrase "inmost self" (much more than language is responsible for this)

but to our having half-persuaded ourselves that this phrase is descriptive and that some one thing should correspond, the same for all people.

When we see this we are sufficiently freed from the influence of the house-model to be able to look at the argument from analogy in a new light. (Or to see it and ourselves in the same light.) We see that, for our belief about how someone is feeling to be testable, it is only required that we should be able to put ourself in what is, sufficiently for the purpose at hand, his situation, the situation in which he finds himself. (If we cannot imaginatively, *i.e.* metaphorically, put ourself in someone else's place we tend not to claim to know how he feels.) Putting oneself into another's situation *is* what corresponds to entering the (real) house and drinking the tea, and not a shadowy copy of something else which corresponds to this.

A very rough diagnosis of the sceptic's ailment. He imagines (but not very clearly) himself trying to find out how another person is feeling. He thinks of all the mistakes he could make in judging what sort of situation the other was in. But he is careful not to mention any of these specifically, since to do this would be to remind himself of the commonsense ways in which such mistakes, however many, could be guarded against, or, if made, remedied. He is thus like one who talks of dark and terrible things without saying what they are or how to avoid them. This other person, who is not Tom or Jack or Sue or Mary but all and none of these at once, he calls "another" and his situation he calls "another's situation", and thereby ensures that he shan't be reminded of commonsense differences between other people, more or less opaque, more or less knowable. He then seizes upon something in the situation which, he supposes, makes it impossible for him (ever) to know about the other's mind, namely that it is *his* situation, and he half-persuades himself and us that to call it this is to *describe* it. Between supposing that this is a description which connotes impenetrability and supposing it to be a variable description which may sometimes describe impenetrable things, we get ourselves into a mess. Also "his situation" *might* be used to refer to a situation into which we could not put ourselves, and "his mind" *might* be used to refer to a mind about which we could know little or nothing. The sceptic's peculiar bluff is to say "You can never know what is going on in another's mind" and to make this sound like "You can never know what is going on in a Chinaman's mind". We call his bluff by saying "Who is this other you talk so much about?"

A word in particular about sensations. We get to know about another's sensations by looking where he's looking, touching what he's touching and so on. And sometimes when we do this we're right in claiming to know how he sees the familiar things that we see, and so on. But not always; for example, if he has jaundice and we do not know we may mistakenly suppose that he sees as white what we see as white (and what he sees as yellow). Brooding on the possibility of more mistakes of this kind that we cannot foresee, we may come to suppose that all people but ourselves suffer from something like jaundice but far far worse, a disease undectable to science and one that we can never catch (or, catching it, be sure that all others have not just been cured). This feeling that sense-data are somehow *tainted* by their metaphysical owners has led to the assertion that sense-data have no owners, and thus escape being tainted. In view of the use of "give" and "*dare*" whereby if something is given there is someone to whom it is given, this is not helpful.

Finally, there is a puzzle about "Smith will feel pain in ten minutes time"; this should mean the same to all those who understand it, yet to Smith it means pain, to Jones, winces. Could we say that it does mean the same to each of Smith and Jones; to each it means that Smith will feel pain; that Smith's understanding the sentence includes knowing who is referred to as Smith, and hence *is* understanding that he will feel pain; and that it is only inasmuch as Jones too knows who is referred to as Smith that he expects winces, if he does, from that person?

London University.

VII.—THE ANALYSIS OF CONDITIONAL SENTENCES

By D. J. O'Connor

Recent discussion of conditional statements has covered a wide variety of different questions which have not always been properly distinguished. Mr. Stuart Hampshire has pointed out[1] that statements like "If Hitler had invaded in 1940, he would have captured London" have presented a problem to three classes of philosophers: (i) those who wish to analyse all complex statements as truth-functions of simple statements in an extensional logic; (ii) those who want to say that the truth of a statement consists in its correspondence with a fact; (iii) those who want to say that we cannot understand any empirical statement unless we can state, in principle, how the statement may be confirmed or refuted. In his own discussion of the question, he considers the difficulties of the third class of philosophers only. In this paper, I want to say something which will be relevant to all three sections of the problem which he outlines.

I shall not confine myself to discussing contrary-to-fact conditionals, however, as the special difficulties of this type of statement can be seen best by contrast with the analysis of ordinary indicative conditionals. It is worth pointing out that not only do conditional sentences raise difficulties for different philosophical assumptions, but also that the "type" of statement is not one simple identifiable "natural kind". There are a number of different sorts of hypothetical statement. They do not all raise the same problems and unless we distinguish them, any discussion of the problems which they raise will be misleading. It is, however, notoriously difficult to outline a satisfactory natural history of statement types. Any such enterprise is bound to omit the essential semiotic nuances which a statement derives from the purpose for which it is used and the context in which we find it. We must therefore rely largely on the grammatical structure of a sentence as a guide to its logical and semiotic powers. And though this is known to be an unreliable guide if it is used incautiously, it is probably the best that we have.

There are two obvious ways of classifying hypothetical statements according to their grammatical structure. Such statements

[1] "Subjunctive Conditionals", *Analysis*, October, 1948.

can be universal: "If any piece of sugar is placed in water, it will dissolve", or particular: "If this piece of sugar is placed in water, it will dissolve". They may also be classified according to the mood of the verbs involved as indicative: "If he comes, he will be late", or subjunctive: "If he were to come, he would be late". The use of the subjunctive in such cases has two distinct but associated purposes. It may often indicate that the antecedent of the hypothetical is false, or believed to be false. This is especially obvious when the statement is expressed in the past tense: If he had received medical treatment, he would have recovered". But it is also used to express belief in a causal or "necessary" connexion between the fact referred to in the antecedent clause of the conditional and the fact referred to in the consequent. In the last example, it is clear that a causal relation is affirmed between the failure to receive medical treatment and the patient's failure to recover. Such a natural connexion between features of the world can, of course, be implied by an indicative hypothetical. I may say to a would-be suicide "If you drink that prussic acid, you will distress your relatives". But the use of the subjunctive is often intended to affirm, for example, that a certain natural connexion between certain classes of events or properties does hold in nature so that if a certain antecedent event, A, which admittedly will not take place, *were* to occur, it *would* be followed by a certain occurrence, C.

The use of the subjunctive mood in such statements does not, of course, invariably evince the user's belief that the antecedent of the hypothetical is false or that a certain natural connexion holds in nature or both. I think it is true to say that where the subjunctive mood of a past tense occurs in the antecedent of a conditional sentence, it is intended to be understood that the antecedent is contrary-to-fact. "If x had occurred, y would have occurred" is properly read as meaning that x in fact did not occur. But if I say "If you were to go to the Oval on Saturday, you would see Surrey play Sussex" I do not imply either that you will not go or that it is a law of nature that Surrey should play Sussex on a certain date. We can, in other words, use subjunctive conditionals to express belief in "natural laws" but also in more fortuitous collocations of events.

I shall discuss first of all certain difficulties in the interpretation of conditional statements which are met by those philosophers who insist on analysing all compound statements as truth functions of their simple components. And I want to try to show that their problems are quite illusory because the belief that conditional statements must be analysed in this way is merely a

pedantic superstition. These difficulties do not concern subjunctive conditionals alone and I shall defer the special problems of the subjunctive conditional until later.

In their discussion of contrary-to-fact conditionals, R. C. Chisholm [1] and F. L. Will [2] use great subtlety and ingenuity in trying to devise a formula for analysing such statements which will not entirely abandon the truth-functional interpretation. And a recent paper by Carl Hempel [3] discusses the difficulty of accounting for the concept of dispositional properties, which is indispensable in the natural sciences, by a truth-functional analysis of hypothetical sentences. On the definition of material implication accepted in modern logic, "if p, then q" is true if either p is false or q is true. And, in consequence, "if p, then q" is false only if p is true and q is false. If we try to use this definition in analysing dispositional properties like "brittle", "soluble" or "elastic", we find that certain absurd consequences follow. Suppose "this glass is brittle" is analysed as "if this glass is dropped, it will break." The hypothetical sentence, if interpreted according to the truth-functional analysis of implication, is true if either the antecedent is false or the consequent is true. It would therefore be true if the glass in question were never dropped. This occasions no difficulty in the case of "brittle". But if we replace "brittle" by "soluble" or "inflammable" the inadequacy of the analysis is clear enough. If "this glass is inflammable" means "if this glass is heated, it will burst into flames" then the sentence is true, on the interpretation we are considering, if the glass is never in fact heated. And this is clearly absurd.

But the absurd conclusion is occasioned only by the absurdity of the assumption from which it follows. Why should we suppose that the expressions of ordinary discourse can be exactly analysed only in one of the canonical forms of contemporary logic? Modern logicians have often, and very properly, criticised the classical logicians for attempting to squeeze all indicative expressions into the strait-jacket of the subject-predicate propositional form. They have pointed to all sorts of mistakes, confusions and inadequacies which have resulted from this assumption. But in seeking to interpret all "if—then" sentences as truth-functional compositions of simple components, they are making

[1] MIND, October, 1946. (Reprinted in a revised version in Feigl and Sellars' *Readings in Philosophical Analysis*, New York, 1949).

[2] MIND, July, 1947.

[3] "Problems and Changes in the Empiricist Criterion of Meaning", *Revue Internationale de Philosophie*, January, 1950.

exactly the same kind of error as the Aristotelians. In so far as logic can be used at all as an instrument of philosophical analysis, it is a sort of cartography of conventional discourse. Logicians map some of the main structural features of language and in doing so, help us to explore its intricacies and become familiar with its powers. But we must not assume that logic is an exact replica of the linguistic territory explored by logicians any more than a map is a full scale model of the country to which it is a guide. We can use logic as a guide in the analysis of conventional expressions only so far as logic follows conventional usage. Those philosophers who are distressed and puzzled because "if—then" sentences do not all seem able to be read as material or formal implications are like cartographers who find a river or a hill which is not marked on their charts and cannot believe their eyes. "It can't really be there", they say, "or the map would show it."

Luckily cartographers do not behave like this but they have a more robust sense of reality than some contemporary philosophers. The belief that the failure of conditional sentences to fit neatly into the truth-functional definition of implication is a mysterious fact that has in some way to be explained is merely a logician's prejudice. If we pay attention to the sense of hypothetical sentences without any doctrinaire preconceptions we can easily see how they can be analysed on a truth-functional pattern without distorting the sense of the expression. A hypothetical statement like "if the glass is dropped, it will break" makes a prediction of a certain event *under a certain condition.* (This is, after all, part of what we mean by calling a statement "conditional".) It says nothing whatever about what will take place if the condition referred to in the antecedent clause is not fulfilled. We can therefore draw up a truth-table for such statements as follows :

p	q	if p, then q
true	true	true
false	true	undetermined
true	false	false
false	false	undetermined

Such statements are true, in other words, when both antecedent and consequent are fulfilled and false when the antecedent is fulfilled and the consequent is not. And when the antecedent is not fulfilled, there is no standard usage for the labels "true"

and "false". In many cases, indeed, it is obviously impossible that there should be such a usage for all combinations of truth-values. In statements like "if he comes, he will be late", "if this metal is put in water, it will sink" and so on, it is clearly impossible for the second row of the truth-table to be satisfied. For it is analytically true that no one can be late unless he does arrive, that nothing can sink in water unless it is put in water and so on. Those philosophers who require a respectable logical canon for every type of sentence will be relieved to notice that the truth-table given above is identical with the truth-table for "quasi-implication" in Professor Reichenbach's three-valued logic.[1]

If the truth-functional analysis of "if—then" sentences in accordance with the definition of material implication is unsuited to indicative conditionals, it is *a fortiori* entirely inappropriate to the analysis of subjunctive conditionals. Yet the truth-table given above, though it satisfies the usual connotation of indicative hypotheticals, is of no use in the analysis of the subjunctive conditional where the subjunctive mood is used to indicate that the antecedent is unfulfilled. A hypothetical statement of the form "if x *were* f, it *would* be g" is intended to be true notwithstanding the admitted falsity of the antecedent.

Such conditional sentences can either be universal, referring to all the members of a certain class or particular, referring to certain members of the class. For example, "all mammals have twelve pairs of cranial nerves" can be read "for all x, if x is a mammal, then x has twelve pairs of cranial nerves". But more usually they are particular statements and they fall into two main classes. Sometimes they are particular cases of a corresponding general hypothetical. We say, for instance, "if this piece of sugar were to be put in water, it would dissolve" because we believe that the truth of the particular hypothetical is guaranteed by the corresponding general statement "for all x, if x is a piece of sugar and is put into water, x will dissolve". But there are other cases like "if the Greeks had lost at Marathon, the Persians would have captured Athens", where there is no obvious general principle from which the statement can be deduced. Mr. Hampshire considers that statements of this type, which are necessary for a wide range of historical and moral discourse, constitute a special problem for those philosophers who maintain that to say that an empirical statement has meaning entails the possibility of specifying the sort of evidence which would verify the statement.

[1] *Philosophical Foundations of Quantum Mechanics*, p. 168.

It seems to me that conditionals of the first type can be disposed of without much difficulty. When we assert a universal contrary-to-fact conditional, we are doing no more than asserting an ordinary general statement of the type "all S is P" or "no S is P" or a combination of such statements. It is, in fact, very uncommon in ordinary discourse to enunciate a *universal* contrary-to-fact hypothetical. And when we do so, it is merely an indirect way of asserting the truth of another universal hypothetical which is *not* contrary-to-fact. "If frogs were mammals, they would have twelve pairs of cranial nerves" is just a roundabout and unusual way of saying "frogs do not have twelve pairs of cranial nerves and mammals do". Thus the use of the subjunctive to indicate that the antecedent is false creates no problem in the case of universal hypotheticals. But, as we have seen, the subjunctive mood is also used to indicate what is variously called a universal or natural or necessary connexion between properties or events designated in the antecedent clause and those designated in the consequent. And this fact creates another difficulty for those who wish to translate such statements into the accepted forms of contemporary logic. This is most easily seen if we take an example of a particular subjunctive conditional: "If this blue litmus paper were put into acid, it would turn red". Professor Broad has suggested, and it seems to me that he is clearly right, that we believe particular statements of this kind because we recognise that they are particular cases of well-established inductive generalisations.[1] In the example given, we believe the proposition to be true because we believe the general statement: "All blue litmus paper turns red in acid" or "for all x, if x is blue litmus paper and is put into acid, then x will turn red." Mr. Chisholm states [2] that this view "does not contribute towards the solution of our problem, however, since it merely reduces subjunctives about individuals to subjunctives about classes". But what is the "problem" of universal subjunctive conditionals? We have seen already that it is most unusual to use this locution to express a counterfactual statement; and that when such expressions do occur, they are clearly elliptical modes of expression for perfectly innocent generalisations. But Mr. Chisholm is concerned here with the use of the subjunctive, not to deny the truth of the antecedent but to imply a "necessary connexion" between antecedent and consequent. And he thinks that there is a problem because it is not possible to make a plausible translation of statements like "all sugar

[1] Examination of MacTaggart's *Philosophy*, I, p. 276.
[2] *Op. cit.*, pp. 488-489.

dissolves in water" into the terminology of a formal implication: "for all x, if x is sugar and is put into water, then it will dissolve". Mr. Chisholm and many other philosophers are unwilling, then, to translate particular subjunctive conditionals in the manner recommended by Professor Broad because we are told that the correct logical translation of "all S is P" is "for all x, if fx, then gx". And this formal implication cannot be a translation of a subjunctive conditional for the following reason. The subjunctive conditional "if this *were to have* property P, it *would have* property Q" refers to a statement about "open" classes, defined intensionally by reference to a certain property. But the formal implication refers to "closed" classes and states that, *as a matter of fact*, all members of the first class are also members of the second. The classes are here defined extensionally by interpreting the universal quantifier "for all x" as the logical product of the class members, $x_1, x_2, \ldots x_n$, where there are, in fact, just n members of the class concerned.

But why should we make this assumption about the "correct" logical translation of "for all x"? There are, no doubt, some expressions for which the suggested translation is a reasonable one but there are many others for which it does not in any way render the content of the expression but, in fact, distorts it in an entirely arbitrary and unjustifiable way. Here again modern logicians are falling into the same kind of error for which they properly rebuked their Aristotelian predecessors. We are told, on the one hand, that to translate general indicative sentences as the classical logicians did is to make unjustifiable assumptions about the existence of members of the classes about which we are talking. "All unicorns are white" entails "some white things are unicorns": and this is clearly absurd. We must therefore translate the sentence more cautiously and read it as "for all x, if x is a unicorn, then x is white". Thus our new translation makes no assertion about the existence of unicorns. But we are also told that we must translate $(x) \,.\, fx$ as $fx_1 \,.\, fx_2 \,.\, fx_3 \ldots fx_n$, where $x_1 \ldots x_n$ are all the members of the class defined by the property f. And this translation at once changes the class F from an open class with an indefinite number of members to a closed class whose members we can enumerate. And classes with members which can be enumerated are certainly classes which have members [1] so that the old Aristotelian error is reinstated in a more sophisticated, though less excusable form.

Thus the first objection to translating particular contrary-to-fact conditionals as instances of general statements can be seen to

[1] With the trivial exception involving null-classes.

rest on a misguided determination to take the conventions of symbolic logic as a safe guide to the meaning of statements in natural languages. There is, however, a further point about the analysis of many such conditionals which has been thought to be a difficulty. Mr. Chisholm describes as "the crux of the whole problem" the difficulty of finding an analysis of subjunctive conditionals which will enable us to distinguish between what he calls "accidental" and "non-accidental" universal conditionals.[1] Suppose I own three dogs and all of them are black. Then I could truthfully say: (A), "for all x, if x is a dog owned by me, then x is black". I could also say: (B), "for all x, if x is a dog owned by me, then x has a brain". But I could not say: (C), "for all x, if x *were* a dog owned by me, then x *would be* black" although I could say: (D), "for all x, if x *were* a dog owned by me, then x *would have* a brain". I could say (D) and not (C), because there seems to be a natural or "necessary" connexion between being a dog and having a brain and to be purely fortuitous *de facto* connexion between being a dog in my possession and being black.

Mr. Chisholm concludes reluctantly, in the face of such instances, that "*connexion* (sc. between attributes) becomes an irreducible ontological category and a source of embarrassment for empiricism".[2] It is always difficult to be sure of the meaning of phrases like "irreducible ontological category" but presumably Mr. Chisholm means that there is a clear distinction between properties which are tied together in nature by a necessary connexion and those which are associated merely fortuitously; and, further, that this distinction can be seen to be a genuine difference in kind. If there were such a generic difference between necessary and accidental connexions, we could recognise its existence only by discerning some characteristic which accompanied all necessary connexions and was absent from all fortuitous ones. And, so far as I am aware, neither Mr. Chisholm nor any other philosopher has ever been able to discern such a differentiating property and establish its existence to the general satisfaction. And this fact in itself creates a strong presumption against the suggested distinction. Indeed, Mr. Chisholm admits [3] that the only way we have of distinguishing between accidental and non-accidental universal conditionals is that the former do not allow us to infer the truth of certain counterfactual hypotheticals and the latter do. And this cannot, of course, be used to establish the alleged ontological distinction between accidental and necessary connexions since

[1] *Op. cit.*, p. 493.
[2] *Op. cit.*, p. 496.
[3] MIND, October, 1946, p. 302.

he distinction was itself postulated to account for the fact that certain general propositions entailed such counterfactual hypotheticals and others did not.

I wish to maintain that the fact that we can all make contrary-to-fact conditional assertions and have good reason for believing them to be true does not in any way entail metaphysical consequences about necessary connexions between properties or "ontological categories". I should have thought, indeed, that one of the few propositions established with reasonable certainty by modern philosophical analysis is that there are no necessary connexions between properties other than those which follow analytically *ex vi terminorum* either by conventional usage or by definition. But this is too extensive a question to argue here.

In any case, I cannot see why this distinction, if it existed, would be a scandal for empiricists. If such connexions were found in experience, they would simply have to be accepted as part of the world. But if Mr. Chisholm means by "empiricist", as he seems to do, one who accepts an extensional logic as a symbolic medium adequate to express all meaningful statements, such necessary connexions would indeed be a serious difficulty for empiricism. For an extensional logic does express the sense of (A) above or any similar sentence involving closed classes and *cannot* express the sense of (B) or any similar sentence involving open classes. But the only comment necessary on this is "so much the worse for an extensional logic". We must not expect any logical system to express all the structural features of ordinary language. And when we realise the limitations of an extensional logic, it is easy to see why we can truly state (D) above and cannot state (C). As (B) is not adequately translatable into the terminology of Principia Mathematica, neither is the counterfactual statement (D) which is derived from it. But why cannot we state (C), which is the counterfactual statement derived from (A)? For (A) can be expressed in an extensional logic. The answer is that there are and can be no counterfactual conditionals corresponding to a statement of type (A) involving reference to closed classes. It is, indeed, self-contradictory to assume that there could be such a statement. For if $(x) . fx$ is shorthand for $fx_1 . fx_2 . fx_3 . \ldots . fx_n$ where there are n enumerable members of the class, the notion that there could be a further true statement fx_{n+1} is excluded by the very meaning of the quantifier. For "(x)" is a summary expression of the fact that there are no more than n members of the class characterised by the property f. Thus a contrary-to-fact conditional *cannot* be expressed in an extensional logic. But this fact does not entail any metaphysical

conclusions about necessary and non-necessary connexions of attributes in nature. It is merely a consequence which follows analytically from the assumptions of the logic itself.

What I wish to conclude is that Professor Broad's analysis of subjunctive conditionals is entirely satisfactory. This may be shown not only by the failure of the arguments in criticism of it but also by the following considerations. Conditional sentences in which the truth-values of the antecedents are unknown are equivalent to ordinary inductive generalisations. We make such statements on certain evidence drawn from our previous experience of the same sort of subject matter. I believe that if I drop this piece of sugar into water, it will dissolve, only because I believe the general statement "all sugar dissolves in water". The crucial point here is that my evidence for believing the particular conditional is *identical with* my evidence for the generalisation.

It may be objected, however, that "if I drop this piece of sugar into water, it will dissolve" is quite unlike a contrary-to-fact conditional in being a respectable type of statement whose truth or falsity we know how to establish. I verify it by making the antecedent come true. And it is certainly the case that if I drop the sugar into water and it does dissolve, I have both completely verified the conditional and increased, though to a negligible extent, the evidence for the generalisation. But *before* I verify the particular statement, it is true to say (*a*) that it is a *meaningful* statement and (*b*) that the empirical facts which give it meaning and constitute the evidence on which we assess its probability are *identical* with the empirical facts which do the same for the generalisation. Thus the meaningfulness of the statement "if I drop this sugar into water, it will dissolve" are in no way dependent on the fact that it can be conclusively verified by making the antecedent true. Its meaning and its probability status are both derivative from the meaning and probability status of the generalisation of which it is an instance. Now if an indicative hypothetical can be true and meaningful in this way, so can a contrary-to-fact hypothetical. For the only difference between "if x happens, y will happen" and "if x had happened, y would have happened" is that the antecedent of the former can be true while the antecedent of the latter cannot. And as this fact in no way affects the meaningfulness and probability status of the indicative conditional, it cannot affect that of the subjunctive conditional either.

I conclude therefore that a particular contrary-to-fact conditional has exactly the same meaning as the corresponding

universal indicative statement. The evidence which establishes one will establish the other and the two verbal forms are merely two different ways of saying the same thing. But though the particular contrary-to-fact conditional is only verbally different from an inductive generalisation, it is only verbally similar to an indicative conditional. It is, in fact, not a genuine hypothetical at all as the "hypothesis" is expressly implied to be untestable. In the case of the indicative conditional, on the other hand, the hypothesis is a real one and can be tested, as the truth-table for "quasi-implication" shows. Thus there can be no genuine problems connected with the contrary-to-fact conditional which do not arise in connexion with inductive generalisations. No doubt there are such problems but they are not the special puzzles which philosophers have examined in connexion with the subjunctive conditional. There is therefore no need to discuss them here.

Before concluding, I would like to add a word about Mr. Hampshire's problem. He was concerned only with those particular judgements in subjunctive form which are not simply related to any empirical generalisation as an instance of the generalisation in question. "If Hitler had invaded in 1940, he would have captured London", "if he had not acted as he did, the disaster would not have occurred" and similar singular historical judgements are examples of such statements. Mr. Hampshire thinks that such statements differ from the contrary-to-fact conditionals of science for which a supporting generalisation can be found in that "the conditions of their falsification are not exactly prescribed".

Nevertheless, it can hardly be denied that we do recognise the difference between a plausible and a fantastic statement of this kind. "If Napoleon had won Waterloo, he would not have died on St. Helena" is a plausible judgement. "If Napoleon had won Waterloo, he would have invaded Iceland" is not. We obviously do have criteria for rating such statements roughly on a scale of plausibility; and this means that we have criteria for verifying and falsifying them in the only sense in which they are susceptible of such confirmation. The only difference between statements of this type and counterfactual hypotheticals of a scientific character is that the relevant evidence for such historical judgements is immensely more complex and more difficult to obtain and put in order. But since we can and do rationally debate their plausibility, there is evidence for such statements by which they can be confirmed and disconfirmed. Just as "if this body had been unsupported, it would have fallen" is just another way of saying

"all unsupported bodies fall" so the judgement about what would have happened had Hitler invaded in 1940 is just a summary reference to the complex mass of historical and military facts and generalisations which we would offer, if challenged, as the "evidence for" the subjunctive conditional.

But in pointing out the distinction between these two kinds of subjunctive conditional, Mr. Hampshire intended to draw attention to the fact that a verification principle which is too exactly or formally stated may easily exclude from the class of meaningful statements historical and moral judgements which we all wish to make and which are most concisely and conveniently expressed in the form of a subjunctive conditional. And, as I have tried to show earlier in this paper, it is indeed important to realise that we must not let a natural desire for precise and formal ways of expressing philosophical truths get the better of common sense.

University of Natal.

VIII.—THE LOGIC OF CAUSAL PROPOSITIONS

By Arthur W. Burks

1. *Introduction*

It is characteristic of a science that growth in its content is accompanied by an increase in the technicality of its language. This is no accident, for the achievements of a well-developed science cannot be completely expressed in the vague, ambiguous, and weak terminology of its early stages. For example, the language of pre-Galilean physics is inadequate to express the modern conceptions of force and energy.

Unfortunately the remark just made about scientific languages does not generally apply to philosophical ones. The notable exceptions are to be found in the fields of logic, both deductive and inductive, where the techniques of symbolic logic have been employed to give precise and technical definitions of such concepts as 'valid inference', 'constructibility', 'intensional', and 'degree of confirmation'. This paper is intended as a contribution along the same lines: it seeks to develop a language for expressing *causal propositions* (contrary-to-fact conditionals, statements of causal laws, assertions of causal necessity and possibility, etc.) which is more precise, explicit, and formal than the language of everyday discourse.

The new language is called the 'logic of causal propositions'. It will be developed by the following procedure. In section 2 various types of causal propositions will be analysed as they occur in ordinary usage. To resolve ambiguities and vaguenesses as well as to simplify the treatment, a new symbolism will be introduced and the sentences being analysed translated into it. The logical principles governing inferences among causal and other propositions will then be formulated. Finally, in section 3, the resultant symbolic language will be organized into a formal system.

It should be made clear at the outset that we are concerned with the deductive interrelations of causal and non-causal propositions, and that hence this is not a study in the logic of confirmation or the theory of probability. As the traditional division of logic into inductive logic and deductive logic

recognizes, there are two different kinds of relations into which causal propositions enter. In other words, to define 'cause', 'causal law', etc., completely, we must formulate both the deductive and the inductive properties of these concepts. In this paper we will deal only with the former.

It might seem that we could best achieve our objective of a technical language for discussing causality by ignoring present usage and starting *ab initio*. This is not the case, however, since the primary motive for constructing a new language is to help answer questions (see the example in the next paragraph) which hitherto have been of necessity phrased in the old language. Unless there is continuity between the new and the old we shall not be answering the original questions but different ones. On the other hand, despite this continuity, there must be important differences between the two languages, else one could not be more technical than the other. Basically, in developing a technical language out of a vague and ambiguous one we must make certain somewhat arbitrary decisions. Also, the symbols of the new language should be chosen so as to be convenient from a formal point of view, whether or not they correspond directly to the basic symbols of ordinary language; though of course the new symbolism should be sufficiently rich to make possible definitions of the old symbols (insofar as they are clear and precise) in terms of them.

The relations which obtain between a technical language and the language of which it is an outgrowth may be illustrated by the following example. Consider the question: Can causal propositions be adequately translated into an extensional language (*e.g.*, that of *Principia Mathematica*)? The first point to note is that this question is a technical reformulation of a very old metaphysical one: Can the concept of causal connexion be defined in terms of ideas of matter-of-fact and constant conjunction, *i.e.*, can causal potentialities be reduced to actualities? This issue was argued by the medieval nominalists and realists, but it is important from our point of view that stating it as they did in a non-technical language they fused and confused it with many other issues: questions concerning the ontological status and value character of properties and concepts, of mathematical principles, and of ethical laws. The second point to note is that just as symbolic logic aids in separating out this one issue of causal laws from all the others, it can profitably be employed to help solve it. For the logic of causal propositions tells us some of the properties of causal propositions, and hence provides criteria for judging any proposed extensional translation.

There is a matter related to this second point which warrants attention. If the question stated in the previous paragraph has an affirmative answer, *i.e.*, if causal propositions are extensional, a knowledge of the extensional analysis of causal propositions would make the development of the proposed logic unnecessary. So it might seem that the answer to the given question should be regarded as a means to the proposed logic, rather than the reverse. Such is not the case, however, as the question under consideration admits of no easy answer. For example, the simple translation of a law in terms of universal quantification and material implication is inadequate (*cf.* 2.4). That an extensional translation, if it exists, is complicated, is shown by the following considerations.

The ultimate data on which causal laws and theories rest—statements reporting the results of observations and experiments—can certainly be expressed in an extensional language. To determine whether or not causal laws and theories are themselves extensional we must examine the relation between them and their evidence. Since the relation between empirical evidence and a law of nature is inductive rather than deductive in character this kind of consideration brings in all the complexities of inductive logic and probability theory. Hence it seems reasonable to begin with a study of the deductive properties of causal propositions.

2. *Analysis of Causal Propositions* [1]

In this section we shall analyse causal propositions and their relations to *extensional propositions* (material implications, ordinary disjunctions, conjunctions, quantified statements, etc.) and *logical propositions* (strict implications, assertions of logical necessity, possibility, impossibility, etc.). The results will be given in the form of logical principles which in section 3 will be organized into a formal system. We shall introduce in this section only those principles governing causal propositions which are not included in the ordinary machinery of existing logic. We intend these new principles merely to supplement those of existing logic, and shall construct the deductive system of section 3 so that it contains both.

[1] In these analyses I am indebted to private discussions with C. H. Langford, Paul Henle, and Irving M. Copi.

2.1. *Contrary-to-Fact Implication*

Consider a hypothetical, contrary-to-fact statement about the past, expressed in the subjunctive mood ; *e.g.*, someone says of a person coming out of a gas-filled basement : ' If he had lit a match the whole house would have exploded '. This is clearly a compound sentence of the ' if . . . then . . .' form. It should not, however, be analysed as ' If (he had lit a match) then (the whole house would have exploded)', for the antecedent and consequent of this sentence are subjunctive rather than indicative and hence are not amenable to existing logics. Rather, the subjunctive element should be placed in the connective, giving ' If it had been the case that (he lit a match) then it would have been the case that (the whole house exploded) '.

The logical manipulation of sentences is simplified if the temporal reference is made by means of a fixed co-ordinate system rather than by the use of verb tenses. Let ' L ' and ' H ' designate the repeatable properties of ' lighting a match ' and ' exploding a house ' and ' a ' the approximate, extended spatio-temporal region referred to. Since the kind of implication involved in this sentence is neither material nor strict (we call it ' contrary-to-fact implication ' or ' counterfactual implication ') a new symbol ' *s* ' is introduced for it. The sentence then becomes ' La *s* Ha '.

There are two important points to note about our symbol ' *s* '. In the first place, we stipulate that ' *s* ' always represents a certain state as a fact, and is never used to express an attitude of wishing or the like. (Contrast in this connexion the subjunctive sentences ' O were he only here ', ' May he return soon ', and ' Part we in friendship from your land '.) In the second place, ' *s* ' is always counterfactual. In contrast, ordinary subjunctive sentences are sometimes counterfactual and sometimes not. The sentence ' if the criminal had come by automobile he would have left tracks ' uttered by a detective as he approaches the scene of a crime is an example of the non-counterfactual subjunctive, as is the sentence ' If they should miss the train they would have to wait an hour at the station '. Since we do not wish our symbolism to be subject to such ambiguity we stipulate that ' *s* ' is always counterfactual and introduce another symbol in 2.2 for the non-counterfactual uses of the subjunctive. We can express the counterfactual character of ' *s* ' by stating that the following is a valid logical principle :

$$(P_1) \quad p\, s\, q\, .\supset \sim p.$$

Further valid rules for ' *s* ' can best be stated in connexion with

the concept of ' causal implication ', and hence will be given in a later subsection (2.3).

It is the function of logic to classify and formulate fallacious forms of argumentation as well as valid ones. Ordinarily this is done by some systematic method; *e.g.*, the use of truth-tables for the propositional calculus. At this stage in the development of the logic of causal propositions, however, such a method is by the nature of the case excluded. Hence we shall employ the older method of listing invalid argument forms (formal fallacies). For example, ' La *s* Ha ' implies neither that ' Ha ' is true nor that it is false, for while ' La *s* Ha ' implies that under the circumstances the truth of ' La ' would have been *sufficient* to insure the truth of ' Ha ' it does not imply that it was necessary (*cf.* 2.2). *E.g.*, the house could have been exploded by the spark from a starting motor. Hence the following statement forms are fallacious (not universally valid):

$$(F_1) \quad p \; s \; q \; . \supset \sim q,$$

$$(F_2) \quad p \; s \; q \; . \supset . \; q \; s \; p.$$

2.2. *Causal Implication*

Consider the following statement of a causal law: 'A beam of electrons moving in a vacuum perpendicular to a magnetic field is deflected'. While we commonly symbolize such a sentence by means of material implication this is not correct (*cf.* 2.4). The implication involved in a causal law is different from both material implication and strict implication. We shall call it ' causal implication ' and introduce the symbol ' *c* ' for it. The given sentence may then be symbolized by ' (x)(Ex *c* Dx)'. We shall call any statement of the form ' (x)(Ex *c* Dx) ' a *causal universal*. Causal implications also occur in statements that are not universal, as ' Ea *c* Da '. A non-counterfactual subjunctive (' If he should release that eraser it would fall ', ' If he were to strike me I would strike him ') is also translated as a causal implication. In such cases there is no strictly logical difference between a sentence expressed in the indicative mood (' If he releases that eraser it will fall ') and the corresponding subjunctive sentence, so (in contrast to ordinary languages) we will use the same symbolic form for them.

It should be noted that those symbolically simple predicates which designate ' dispositions ', ' potentialities ', or ' powers ' contain causal implications implicitly. Thus ' That diamond was hard ' implies ' If an attempt had been made to scratch that diamond with, *e.g.*, steel, it would have failed ' and ' That table

is green ' implies ' If white light were shone on the table green light would be reflected '. All predicates except those designating immediately experienced qualities (*e.g.*, as in ' This book appears red now ') are of this kind.

We intend ' Ea *c* Da ' to assert that the conditions expressed by ' Ea ' are *causally sufficient* to make ' Da ' true. By ' sufficient conditions ' we mean a set of conditions, complete with respect to negative properties as well as positive ones (*i.e.*, counteracting causes must be explicitly mentioned) sufficient to cause the state of affairs expressed by the consequent. Thus ' (x)(Ex *c* Dx) ' should be interpreted as ' A beam of electrons moving in a vacuum perpendicular to a magnetic field *and subject to no other forces* is deflected '. There are a number of consequences of the fact that ' ***c*** ' means ' causally sufficient ' that need to be noted.

The first is that the argument from ' Ea *c* Da ' to ' Da *c* Ea ' is fallacious, and hence also

$$(F_3) \quad p\,c\,q \,.\supset.\, q\,c\,p.$$

The second consequence is that the antecedent may contain superfluous or even irrelevant conditions ; *e.g.*, ' A beam of electrons of *one* milliampere intensity moving in a vacuum perpendicular to a magnetic field and *parallel* to a nearby wooden rod is deflected ' [' (x)(ExOxPx . *c* Dx) '] is true. In fact, on this interpretation of the symbolism these are valid inferences :

$$\text{Ea}\,c\,\text{Da}. \quad \therefore\ \text{EaOa} \,.\, c\,\text{Da}.$$

$$(x)(\text{Ex}\,c\,\text{Dx}). \quad \therefore\ (x)(\text{ExPx} \,.\, c\,\text{Dx}).$$

The leading principles of these inferences are

$$(P_2). \quad p\,c\,q \,.\supset: \text{pr} \,.\, c\,q,$$

$$(P_3). \quad (x)(\text{fx}\,c\,\text{gx}) \supset (x)(\text{fxhx} \,.\, c\,\text{gx}).$$

Because of these two principles ' *c* ' may not correspond to the ordinary use of ' cause ' in certain respects. Thus the phrase ' causal law ' is probably not customarily applied to a true causal universal such as ' (x)(ExOxPx . *c* Dx) ' since the antecedent contains superfluous and irrelevant conditions. Also, by (P_2) we can validly infer ' Ea $\sim$ Ea . *c* Da ' from the true statement ' Ea *c* Da ', and it is doubtful whether ordinary usage would sanction this inference (*cf.* 2.5). Fortunately it is not necessary for us to decide exactly what ordinary usage is on these points, for whatever the utility of the ordinary concept of ' causal law ' in everyday affairs it has neither the precision nor the logical simplicity requisite for a basic concept in our system.

The third consequence of the fact that ' *c* ' means ' causally sufficient ' is that it expresses a transitive relation :

(P_4). $p\,c\,q\,.\,q\,c\,r : \supset .\, p\,c\,r,$

(P_5). $(x)(fx\,c\,gx)\,.\,(x)(gx\,c\,hx) : \supset (x)(fx\,c\,hx).$

Note that the acceptance of these principles allows us to predicate the relation of causal implication of events which are not contiguous in time and space. (In such cases, of course, there is a certain oversimplification in our use of the same individual variable or constant in both the antecedent and the consequent of an implication.) In fact, we do not intend '*c*' to connote temporal sequence at all; *i.e.*, it may be used to express causal relations of a non-temporal sort (*e.g.*, Ohm's Law, the Exclusion Principle). As a result transposition is valid:

$$(P_6) \quad p\,c\,q\,.\supset .\sim q\,c\sim p,$$

for if a cause is sufficient to produce an effect then the effect is a causally necessary condition of the cause and the absence of the effect causally implies the absence of the cause. Similarly we have:

$$(P_7) \quad pq\,.\,c\,r : \supset : p \sim r\,.\,c \sim q.$$

Because of these deviations some of the arguments which are valid by the rules of the logic of causal propositions do not sound valid when expressed in ordinary languages. Thus 'If it rains he'll wear his raincoat; therefore if he doesn't wear his raincoat it won't rain' sounds invalid; rain could cause him to wear a raincoat but his not wearing a raincoat has no causal influence on the weather. Nevertheless if he does not wear a raincoat we can infer on causal grounds from the given premise that it won't rain. The matter is further complicated by the fact that such assertions of causal implication are elliptical, many of the relevent conditions being omitted from the antecedent; this point will be taken up later in this section.

The fourth consequence of the fact that '*c*' means 'causally sufficient' has to do with the validity of the principle of exportation.[1] Suppose two conditions are causally sufficient to produce an effect. For example, if an object satisfies the two conditions of being heavier than air and being released, it falls

$$[(x)(HxRx\,.\,c\,Fx)].$$

It does not follow either causally or materially from the fact that one condition is satisfied that the second is causally sufficient to produce the effect. Thus

$$HaRa\,.\,c\,Fa,\ Ha. \quad \therefore\ Ra\,c\,Fa$$

[1] A. W. Burks, and I. M. Copi, "Lewis Carroll's Barber Shop Paradox", MIND, vol. 59 (1950), pp. 219-222.

is an invalid argument, and so this statement form is fallacious:

$$(F_4) \quad pq \,.\, c r : \supset : p \supset . \, q \, c r.$$

On the other hand,

$$(P_8) \quad pq \,.\, c r : \supset : p c . \, q \supset r$$

is a valid statement form, for it does follow, and on causal grounds, that where the first condition is satisfied the second will in fact produce the effect.

An important implication is that statements of ordinary usage involving causal implication must be construed as elliptical to avoid this fallacy. Suppose someone asserts 'If this object is released here and now (a) then (causally) it will fall', when in fact it is true that 'Ha'. It might seem that we should translate this statement into 'Ra *c* Fa' and regard it as having been derived from the premises 'HaRa . *c* Fa' and 'Ha'. But the suggested argument commits fallacy (F_4). To avoid this fallacy we must regard the English sentence as elliptical for a longer statement whose antecedent mentions or refers to conditions sufficient to bring about the consequent. An elliptical sentence is necessarily vague, so that it is impossible to decide with certainty what its meaning is. A plausible translation of 'If this object is released here and now (a) then (causally) it will fall' is 'HaRa . *c* Fa'; *i.e.*, the given sentence is elliptical for a statement which explicitly states conditions sufficient to cause the consequent. It seems clear that most ordinary assertions of causal implication are elliptical in this or a closely similar way.

2.3. *Causal Implication and Contrary-to-Fact Implication*

We can further explicate the meanings of '*c*' and '*s*' by considering their interrelations. A typical use of a causal law is to infer a contrary-to-fact implication from it:

$$(x)(\mathrm{Ex}\, c\, \mathrm{Dx}), \quad \sim \mathrm{Ea} \quad \therefore \ \mathrm{Ea}\, s\, \mathrm{Da}.$$

Thus there is a valid form of inference involving the denial of the antecedent of a causal implication:

$$(P_9) \quad p c q \,.\, \sim p : \supset . \, p s q.$$

Similarly,

$$\sim \mathrm{Ea}, \quad \sim (\mathrm{Ea}\, s\, \mathrm{Da}) \quad \therefore \ \sim (x)(\mathrm{Ex}\, c\, \mathrm{Dx})$$

and

$$(P_{10}) \quad \sim \mathrm{fy} \,.\, \sim (\mathrm{fy}\, s\, \mathrm{gy}) : \supset \sim (x)(\mathrm{fx}\, c\, \mathrm{gx})$$

are valid.

It is natural to ask: Does the converse of (P_9) hold? Equivalently, is it a valid principle that

$$(P_{11}) \quad p s q \,.\, \equiv : \sim p \,.\, p c q \,?$$

It does seem that when we use the subjunctive in the counterfactual sense ('s') we mean to be asserting both a causal relation ('c') and to be denying that the antecedent is true. A more general sort of confirmation of the validity of (P_{11}) is to be found in the intuitively acceptable character of the various principles that it implies. We note first that it does not conflict with any of the previous valid or invalid statement forms involving 's'. Also, (P_{11}) leads to the following transitivity relations which seem intuitively correct:

$$(P_{12}) \quad p\,s\,q\,.\,q\,s\,r : \supset .\, p\,s\,r,$$
$$(P_{13}) \quad p\,s\,q\,.\,q\,c\,r : \supset .\, p\,s\,r.$$

Further consequences will be developed later.

We saw before (2.2) that most ordinary uses of causal implication are elliptical. (P_{11}) implies that the same must be true of contrary-to-fact implication. As before, it is impossible to decide exactly what the meaning of an elliptical sentence is, but a plausible translation of such a sentence as 'If this object had been released then and there (a) it would have fallen' is 'If this object had been released then and there and had been heavier than air it would have fallen' ['HaRa . s Fa']. Another possible translation is ''Ra' and some of the true propositions about the particular situation (a) imply 'Fa' in a *special contrary-to-fact* sense', where the italicized phrase is defined as follows. An antecedent implies a consequent in this special contrary-to-fact sense when (1) the antecedent is false, (2) the antecedent causally implies (*c*) the consequent, (3) the antecedent does not strictly imply the consequent (*cf.* 2.5), and (4) the antecedent is causally possible and the consequent is not causally necessary (*cf.* 2.5).[1]

A similar problem of translation obtains with respect to subjunctive sentences of the form 'If f had been the case, g would have been the case anyway' or 'Even if f had been the case g would have been the case'; *e.g.*, 'Even if this object had been blue at a it would have fallen'. Such a sentence is asserted when it is believed that sufficient conditions would have been fulfilled to cause the phenomenon even if the one in question had been different. Thus a plausible translation of it is 'If this released object had been heavier than air and blue it would have fallen' ['HaRaBa . s Fa']. Note that this follows from 'HaRa . s Fa', for (P_{11}) and (P_2) imply by ordinary logical rules

$$(P_{14}) \quad p\,s\,q\,.\supset : pr\,.\,s\,q.$$

[1] In this connexion see Nelson Goodman, "The Problem of Counterfactual Conditionals", *The Journal of Philosophy*, vol. 44 (1947), pp. 113-128, part II.

Other possible translations are 'HaRaBa . s Fa : Fa $\sim$ Ba' and '$\sim$(Ba $s \sim$ Fa) . $\sim$ Ba'. The first of these, together with the assertion that 'HaRaBa' is causally possible (*cf.* 2.5), implies the second (by the calculus of section 3). It should be emphasized that we are suggesting only plausible translations of the English sentences under consideration, and are not attempting to give a precise analysis of them. Precise analysis is not required for our purpose, since the analyses we give are sufficiently close to correct usage to satisfy the continuity requirement stated in section 1.

2.4. *Causal Implication and Material Implication*

A very common use of a causal universal is to make predictions from it. For example,

$$(x)(\mathrm{Ex}\ c\ \mathrm{Dx}),\quad \mathrm{Ea}\quad \therefore\ \mathrm{Da}$$

and

$$(x)(\mathrm{Ex}\ c\ \mathrm{Dx}),\quad \sim \mathrm{Da}\quad \therefore\ \sim \mathrm{Ea}$$

are valid arguments. And of course these are valid whether 'a' refers to the future, as in the case of a prediction, or to the past. When a prediction is disconfirmed the causal universal is thereby refuted:

$$\mathrm{Ea},\quad \sim \mathrm{Da}\quad \therefore\ \sim (x)(\mathrm{Ex}\ c\ \mathrm{Dx}).$$

These arguments can all be validated by the principle that a causal implication implies a material implication:

$$(\mathrm{P}_{15})\quad \mathrm{p}\ c\ \mathrm{q}\ .\supset .\ \mathrm{p} \supset \mathrm{q}.$$

This implies

$$(\mathrm{P}_{16})\quad (x)(\mathrm{fx}\ c\ \mathrm{gx}) \supset (x)(\mathrm{fx} \supset \mathrm{gx});$$

in our terminology: a causal universal implies the corresponding *material universal.*

It is natural to ask: Do the converses of (P_{15}) and (P_{16}) hold? Note that if they did, a causal implication would be equivalent to a material implication and a causal universal would be equivalent to the corresponding material universal, with the result that the logic of causal propositions would reduce in immediate and simple fashion to an extensional logic (*cf.* section 1). Good arguments can be advanced, however, to show that these equivalences are fallacious:

$$(\mathrm{F}_5)\quad \mathrm{p}\ c\ \mathrm{q}\ .\equiv .\ \mathrm{p} \supset \mathrm{q},$$
$$(\mathrm{F}_6)\quad (x)(\mathrm{fx}\ c\ \mathrm{gx}) \equiv (x)(\mathrm{fx} \supset \mathrm{gx}).$$

This is the case even though these distinctions are not reflected by ordinary grammatical form. Thus the English 'if . . . then . . .' may be used indifferently for causal implication and

material implication (as well as for strict implication); the kind of implication involved is determined from the content of the sentence or its context.

The argument to show that (F_5) is invalid is simple. For if (F_5) were valid a false proposition would causally imply any proposition, and hence by (P_9) counterfactually imply any proposition. *E.g.*, 'This glass was not released' would imply both '(Releasing this glass) *c* (it exploded)' and '(Releasing this glass) *c* (it didn't explode)', and hence both 'If this glass had been released it would have exploded' and 'If this glass had been released it would not have exploded'. A separate argument is required to establish the invalidity of (F_6). For one could hold that a causal universal is equivalent to the corresponding material universal, but still hold that something more is meant by a causal implication than a mere material implication. *E.g.*, one could hold that '$Ea\ c\ Da$' is true if and only if '$(x)(Ex \supset Dx)$' is true. Such a position appears logically arbitrary, to be sure, but it is a possible one. To show that (F_6) is invalid we point out two unacceptable consequences which follow from the assumption of its validity.

(1) Consider the following true proposition: 'All the books on this desk are in German'. It is clearly a mere summary of facts, a universal proposition true by accident, not by law, so we would naturally symbolize it by '$(x)(Bx \supset Gx)$' rather than by '$(x)(Bx\ c\ Gx)$'. Moreover, we would say that since it is true by accident and not by law the corresponding *contingent universal* '$(x)(Bx \supset Gx) . \sim (x)(Bx\ c\ Gx)$' is true. But on the theory that a causal universal is equivalent to the corresponding material universal the quoted statement is self-contradictory!

(2) Suppose next that 'Russell's *Principles* is not a book on this desk' ['$\sim Ba$'] is also true. Under the proposed equivalence we could validly conclude 'If Russell's *Principles* were on this desk it would be in German' ['$Ba\ s\ Ga$']. For we would be given '$(x)(Bx \supset Gx)$', and that it is equivalent to '$(x)(Bx\ c\ Gx)$', so we could infer by ordinary logical rules '$(x)(Bx\ c\ Gx)$'. We could specify this to get '$Ba\ c\ Ga$' which we could combine with '$\sim Ba$' to get '$Ba\ c\ Ga . \sim Ba$'. Then by (P_9) we could infer '$Ba\ s\ Ga$'. Hence if (F_6) were valid we would be unable to distinguish those cases where we can validly deduce contrary-to-fact implications (*cf.* 2.3) from those where such a deduction cannot be validly made.

There is one feature of the examples just employed in (1) and (2) which merits attention. Consider the sentence 'All the books on this desk are in German'. It was translated as

'$(x)(Bx \supset Gx)$', where the predicate 'B' means 'being a book on *this* desk *now*'. This predicate makes reference to a particular place and time, and in this respect is to be contrasted with such a predicate as 'E' ['being an electron beam'] which need not include in its definition any restriction to a particular spatiotemporal region. On this basis we call 'B' an *indexical predicate* and 'E' a *symbolic predicate.*[1] The relevance of this distinction to the matter at hand is that one might argue that though (F_6) is a fallacy in general, it is a valid principle in a language which contains no indexical predicates. (This would mean that it would be a valid principle in a scientific language, for since science investigates only repeatable qualities a scientific language need contain no indexical predicates.)

We can, however, construct examples to show that (F_6) is a fallacy even when the predicates involved are non-indexical. Thus we could modify the above example by replacing the indexical predicate 'B' by a complicated symbolic predicate '$\bar{B}$' such as 'being a book on a desk which is of such a size and shape, which is five feet from an individual with such and such a history, . . ., . . .'. By making this description sufficiently complicated we could make it highly probable that it was unique to this particular desk. Then '$(x)(\bar{B}x \supset Gx)$' would very probably be true, while '$(x)(\bar{B}x\ c\ Gx)$' would very probably be false. The fact that we could not be certain of the uniqueness of the property defined does not destroy the example, because the truth of either a causal universal or a material universal is never known with more than probability.

2.5. *Causal Implication and Strict Implication*

A causal implication implies a material implication (P_{15}) but not *vice versa* (F_5); in contrast, a strict implication implies a causal implication but not *vice versa.*[2] These facts are expressed by the principle

$$(P_{17}) \quad p \dashv\!\subset q \,.\supset.\, p\ c\ q,$$

and the fallacy

$$(F_7) \quad p \dashv\!\subset q \,.\equiv.\, p\ c\ q.$$

It should be noted that because of (P_{17}), 'c' differs in meaning

[1] For an elaboration of this distinction see A. W. Burks, "Icon, Index, and Symbol", *Philosophy and Phenomenological Research*, vol. 9 (1948-49), pp. 673-689.

[2] For logics of strict implication see C. I. Lewis and C. H. Langford, *Symbolic Logic;* Ruth C. Barcan, "A Functional Calculus of First Order Based on Strict Implication", *The Journal of Symbolic Logic*, vol. 11 (1946), pp. 1-16; and Rudolf Carnap, *Meaning and Necessity.*

from the ordinary usage of 'cause' in that the ordinary usage signifies that the implication is causal (in our sense of 'c') but not strict, *i.e.*, the ordinary meaning of 'p causally implies q' should probably be symbolized as '$p\,c\,q\,.\sim(p \dashv q)$'. Our reasons for introducing this principle—apart from its intrinsic plausibility—are as follows.

Adding (P_{17}) to our already existing logical machinery is the simplest way of handling those contrary-to-fact implications and non-counterfactual uses of the subjunctive that are based on strict implications. Thus any statement of the form 'If the premises had been true the conclusion would have been true' follows from the validity of the argument and the falsity of the premises. Likewise, in deducing the practical consequences of a belief we may say 'If this were true than such and such would be the case', which is probably best symbolized by means of 'c'. Again, such a sentence as 'If the Rock of Gibraltar were a man it would be rational' ['G s R'] can be dealt with by means of (P_{17}), for it would follow from the strict implication 'G $\dashv$ R' (assuming the traditional definition of 'man') and the factual premise '$\sim$G' by (P_{17}) and (P_{11}).

A second reason for introducing (P_{17}) is that it takes the place of several more specialized principles. Consider, for example,

$$p\,s\,q\,.\,q \dashv r : \supset .\, p\,s\,r,$$

$$p\,s\,q\,.\supset : p\,s\,.\,pq.$$

These principles are intuitively valid, and by means of them we can validate the argument: 'If Russell's *Principles* were placed on this desk it would not be in German. ∴ If Russell's *Principles* were placed on this desk it would not be the case that all the books on this desk are in German'

['Ba $s \sim$ Ga, ∴ Ba $s \sim$ (x)(Bx $\supset$ Gx)'].

For the second principle allows us to infer 'Ba s. Ba $\sim$ Ga' from the premise, and since

'Ba $\sim$ Ga . $\dashv \sim$(x)(Bx $\supset$ Gx)'

is true, the conclusion follows by the first principle. But this argument can also be validated by making use of the more general principles (P_{17}) and the first of the following two valid principles of distribution.

(P_{18}) $\quad p\,c\,q\,.\,p\,c\,r : \equiv : p\,c\,.\,qr,$

(P_{19}) $\quad p \vee q\,.\,c\,r : \equiv : p\,c\,r\,.\,q\,c\,r.$

The line of argument is then as follows. By (P_{17}) we infer 'Ba c Ba' from 'Ba $\dashv$ Ba'. By (P_{11}) we infer '$\sim$Ba' and 'Ba $c \sim$ Ga' from the premise. 'Ba c Ba' and 'Ba $c \sim$ Ga'

give ' Ba c . Ba $\sim$ Ga ' by (P_{18}), and this with ' $\sim$ Ba ' gives ' Ba s . *Ba* $\sim$ Ga ' by (P_{11}). Using (P_{17}) again we infer ' Ba $\sim$ Ga . $c \sim (x)(Bx \supset Gx)$ ' from the corresponding strict implication. The conclusion follows by (P_{13}).

One consequence of (P_{17}) which must be noted is that it introduces causal paradoxes derivable from the so-called paradoxes of strict implication. Thus because a contradiction strictly implies anything ' Ba $\sim$ Ba . $\dashv$ Ga ' is true and hence by (P_{17}) ' Ba $\sim$ Ba . c Ga ' is true; since ' $\sim$ (Ba $\sim$ Ba) ' is true on logical grounds we may infer ' Ba $\sim$ Ba . s Ga ' by (P_{11}). Similarly, ' Ba c . Ga $\vee \sim$ Ga ' is true. These results have a certain sanction in ordinary usage; *e.g.*, ' If Russell's *Principles* were placed on this desk it would either be in German or not in German ' does not seem an unreasonable consequence of ' Russell's *Principles* is not on this desk '. In any event, as we have previously noted there is considerable arbitrariness inherent in the symbolic formulation of any common-sense idea. Once a case of vagueness is posed clearly by means of a precise symbolism its resolution should be determined by convenience. It is on this basis that we adopt (P_{17}).

There is a consequence of introducing the paradoxes of strict implication into the logic of causal propositions which is of considerable importance. Because of these paradoxes statements of the form ' p c q ' and ' p $c \sim$ q ' are not simple contraries; *e.g.*, ' Ba $\sim$ Ba . c Ga ' and ' Ba $\sim$ Ba . $c \sim$ Ga ' are both true. Thus it is a fallacy that

$$(F_8) \quad p\, c\, q \,.\supset \sim (p\, c \sim q).$$

Similarly it is a fallacy that

$$(F_9) \quad p\, s\, q \,.\supset \sim (p\, s \sim q).$$

In order to state the circumstances under which ' p c q ' and ' p $c \sim$ q ' (and ' p s q ' and ' p $s \sim$ q ') are contraries it might seem that we should qualify the above statement forms by stipulating that they hold only if the antecedent of each causal (or contrary-to-fact) implication is logically possible (not self-contradictory) and the consequent of each such implication is not logically necessary. This qualification would exclude the cases we have been considering so far. But there are also " paradoxical " cases to be considered which are not derived from the paradoxes of strict implication though they are parallel to them.

Corresponding to the implications with logically impossible antecedents there are implications with causally impossible antecedents, *i.e.*, antecedents which can never be exemplified because

they contradict existing laws. *E.g.*, assuming '$(x)(RxHx \,.\, c\,Fx)$' to be true, '$RaHa \sim Fa \,.\, s\,Wa$' ['If this released, heavier than air, object had not fallen it would have been white'] and '$RaHa \sim Fa \,.\, c\,Wa$' are such implications. Similarly, corresponding to the implications with logically necessary consequents there are implications with causally necessary consequents, *i.e.*, consequents true by virtue of a causal law. Under the same assumption as before '$Wa\,c : RaHa \,.\supset Fa$' ['If this object is white then (causally) if it is released and is heavier than air it will fall'] is such an implication. It seems desirable to count these statements as true, rather than false, just as we count the corresponding strict implications true. We shall do this, though we emphasize that there is a certain arbitrariness in this procedure.

Thus there are "paradoxes" of causal implication analogous to the paradoxes of strict implication: a causally impossible antecedent causally implies any consequent and a causally necessary consequent is causally implied by any antecedent. To state these principles symbolically we introduce symbols for the concepts of causal possibility ('$\diamondsuit_c$') and causal necessity ('$\Box_c$'),[1] corresponding to the concepts of logical possibility ('$\diamondsuit$') and logical necessity ('$\Box$'). The "paradoxes" of causal implication are then

$$(P_{20}) \quad \sim \diamondsuit_c p \supset . p\,c\,q,$$

$$(P_{21}) \quad p \supset . q\,c\,p.$$

With these causally modal concepts we can also state the principles of contraries for causal and contrary-to-fact implications:

$$(P_{22}) \quad \diamondsuit_c p \supset \sim (p\,c\,q \,.\, p\,c \sim q),$$

$$(P_{23}) \quad \diamondsuit_c p \supset \sim (p\,s\,q \,.\, p\,s \sim q).$$

(It is unnecessary to require that 'p' be logically possible as well as causally possible, for as we shall see by (P_{28}) that which is causally possible is also logically possible.)

Lewis Carroll's barber shop paradox[2] provides an example of the use of the causal modalities. The owner of the barber shop lays down the following rules: (1) 'If Carr and Allen are out, then Brown must be in' ['$CA \,.\, c \sim B$'], and (2) 'If Allen goes out Brown must accompany him' ['$A\,c\,B$']. Carroll's argument is as follows: (a) the first rule implies '$C \supset . A\,c \sim B$', (b) the second rule implies '$\sim (A\,c \sim B)$', and (c) by *modus tollens*

[1] These are what Hans Reichenbach (*Elements of Symbolic Logic*, p. 392) calls physical modalities.

[2] Burks and Copi, *op. cit.*

these results imply '$\sim$C'; *i.e.*, the given rules imply that Carr can never go out! Step (a) is of course invalid by (F_4) and step (b) is valid only if the true premise '$\Diamond_c$A' is assumed (*cf.* P_{22}). It should be noted that 'AC', together with the two rules, implies a contradiction by (P_{15}) and the usual rules of logic. 'AC' is clearly logically possible; therefore it must be causally impossible. Hence we have the valid principle

$$(P_{24}) \quad pq \,.\, cr : pc \sim r : . \supset \sim \Diamond_c(pq).$$

Thus the troublesome feature of Carroll's example is that the rules are such that the antecedent of one represents a causal impossibility. (Of course, this is true only on the assumption that the rules laid down are obeyed, and hence our symbolization of Carroll's rules is not completely adequate. If they do obey the rules, the barbers have certain causal dispositions—as is shown by the fact that counterfactual propositions may under appropriate circumstances be inferred—so that an adequate symbolization will be of the general form given.)

The concepts of causal necessity and possibility can be related to the concept of causal implication, just as the corresponding ideas of logical possibility and necessity can be related to the concept of strict implication. Thus we have

$$(P_{25}) \quad \Box_c(p \supset q) \equiv . \, pcq.$$

Causal possibility is related to causal necessity in this way:

$$(P_{26}) \quad \Diamond_c p \equiv \sim \Box_c \sim p \,.$$

When (P_{25}) is applied to a causal universal the symbol for causal necessity ('$\Box_c$') appears inside the scope of a universal quantifier in the resultant expression. *E.g.*, '(x)(Ex *c* Dx)' becomes '(x) $\Box_c$ (Ex $\supset$ Dx)'. The question arises as to the proper interpretation of this result. Now to say that for every spatio-temporal region 'Ex $\supset$ Dx' is true on causal grounds is logically equivalent to saying that '(x)(Ex $\supset$ Dx)' is true on causal grounds. Thus we have [1]

$$(P_{27}) \quad (x)\Box_c fx \equiv \Box_c(x)fx.$$

We are now in a position to compare the properties of the causal symbols we have introduced with those of the corresponding logical symbols. The first point to note is that while a causal implication or necessity is implied by the corresponding strict implication or logical necessity, it implies the corresponding material implication or assertion without a modal operator.

[1] *Cf.* Carnap, *op. cit.*, pp. 178-179, 186.

This is exemplified by (P_{15}) and (P_{17}) and also by

(P_{28}) $\Box p \supset \boxed{c} p$,

(P_{29}) $\boxed{c} p \supset p$.

(Note that by (P_{28}) the paradoxes of strict implication become special cases of the paradoxes of causal implication.) In the second place, the deductive interrelations of 'c', '$\boxed{c}$', and '$\diamondsuit_c$' are quite similar to those of '$\sim$', '$\Box$', and '$\diamondsuit$'. This has already been illustrated by the parallelism of the causal modalities and the logical modalities. It is further verified by the fact that this hypothesis accounts for all the rules of the present section (as we shall show in section 3) and for all the fallacies (in the sense that the fallacies are not theorems). Thus it would seem that causal propositions have a certain modal character, despite the fact that they are empirical rather than rational. This suggests that the formalized logic of causal propositions should be a double modal logic. We now turn to the construction of such a system.

3. *The Calculus of Causal Propositions*

In this section we shall develop an applied functional calculus of first order which will formalize the results of section 2. This calculus is patterned after Church's formulation of a functional calculus of first order and Fitch's development of a modal logic with quantifiers.[1]

The limitations of our language should perhaps be commented on. A language of first order is clearly inadequate to state the statistical and quantitative theories of a well-developed science. We believe, however, that besides being a first step toward achieving a truly adequate language, the present formulation is of intrinsic interest because many of the questions about causality traditionally considered by philosophers (*e.g.*, Bacon, Hume, Mill, and Keynes) concern universal (non-statistical), qualitative laws.

3.1. *Vocabulary and Formation Rules*

The extensional part of the primitive vocabulary consists of the logical constants $\sim$, $\supset$, parentheses, and the universal quantifier; and of infinite lists of propositional constants and variables, individual constants and variables, and functional constants and variables of all degrees. This is supplemented by

[1] Alonzo Church, *Introduction to Mathematical Logic*, part I, and F. B. Fitch, "Intuitionistic Modal Logic with Quantifiers", *Portugaliae Mathematica*, vol. 7 (1948), pp. 113-118. Consult these works for historical references to the methods employed.

the non-extensional primitive symbols $\Box$ and $\boxed{c}$. Symbols of the object language will generally be used autonomously as in the preceding sentences. Capital Roman letters and lower-case Greek letters, with or without numerical subscripts, will be used as variables in the syntax language, Roman letters taking formulas of the calculus as values and Greek letters taking individual variables as values unless otherwise specified.

A formula is any finite sequence of primitive symbols. The recursive definition of 'well-formed formula' ('w.f.f.') is:

A propositional variable or constant alone is w.f.

If β is a functional variable or constant of degree n and $\alpha_1, \alpha_2, \ldots, \alpha_n$ are individual variables or constants, $\beta(\alpha_1, \alpha_2, \ldots \alpha_n)$ is w.f.

If A and B are w.f., $\sim$A, (A $\supset$ B), (α)A, $\Box$A and $\boxed{c}$A are w.f.

No formula is w.f. unless its being so follows from these rules. Hereafter the Roman letter syntactic variables will take as values only w.f.f. Parentheses will be omitted or replaced by dots whenever this simplifies the punctuation without introducing ambiguity. An occurrence of an individual variable α in a w.f.f. A will be called 'a bound occurrence' if it is in a w.f. part of the form (α)B; otherwise it will be called 'a free occurrence'.

It should be noted that by these formation rules formulas with modal operators operating on modal operators are w.f., *e.g.*, '$\boxed{c}(\Box p \supset \boxed{c}p)$'. In connexion with the interpretation of these formulas it should be remembered that because of (P_{17}) and (P_{28}) $\boxed{c}$ is not to be interpreted as 'it is true on causal grounds' but rather as 'it is true on either causal or logical grounds'. Examples of sentences with causal modalities operating on causal modalities are sentences which assert that the appearance of one dispositional property causes the appearance of another, or that if a certain causal law were false such-and-such would be the case, or that it is a causal law that a given kind of habit or resolution arises under certain circumstances.

The definitions are:

$$
\begin{array}{lll}
(A \vee B) & =_{df} & (\sim A \supset B) \\
(A \,.\, B) & =_{df} & \sim(\sim A \vee \sim B) \\
(A \equiv B) & =_{df} & ((A \supset B) \,.\, (B \supset A)) \\
(A \prec B) & =_{df} & \Box(A \supset B) \\
(A \; c \; B) & =_{df} & \boxed{c}(A \supset B) \\
(A \; s \; B) & =_{df} & (\sim A \,.\, (A \; c \; B)) \\
(\exists\alpha)A & =_{df} & \sim(\alpha) \sim A \\
\Diamond A & =_{df} & \sim\Box \sim A \\
\Diamond\!\!\!\boxed{c} A & =_{df} & \sim\boxed{c} \sim A
\end{array}
$$

3.2. *Axioms and Primitive Rule of Inference.*

Primitive rule :

(I) From A and A ⊃ B to infer B (*modus ponens*).

The axioms are determined by the following schemata and rules :—

(1) A ⊃ . B ⊃ C : ⊃ : A ⊃ B . ⊃ . A ⊃ C.

(2) A ⊃ . B ⊃ A.

(3) ∼A ⊃ ∼B . ⊃ . B ⊃ A.

(4) (α)(A ⊃ B) ⊃ . A ⊃ (α) B if α is not free in A.

(5) (α)A ⊃ B where α is an individual variable, β is an individual variable or constant, no free occurrence of α in A is in a w.f. part of A of the form (β)C, and B results from the substitution of β for all free occurrences of α in A.

(6) □A ⊃ ⓒA.

(7) ⓒA ⊃ A.

(8) (α)(A ⊃ B) ⊃ . (α)A ⊃ (α)B.

(9) □(A ⊃ B) ⊃ . □A ⊃ □B.

(10) ⓒ(A ⊃ B) ⊃ . ⓒA ⊃ ⓒB.

(11) (α)□A ⊃ □(α)A.

(12) (α)ⓒA ⊃ ⓒ(α)A.

(II) If A is an axiom, so is (α)A.

(III) If A is an axiom, so is □A.

Axiom schemata (9) and (11) are not used in establishing the principles of section 2, but are included for the sake of completeness. By means of them the strict implication analogues of our theorems can be proved. Hence this calculus contains a system of strict implication as well as both an extensional system and a system of causal implication.

3.3 *Theorems*

'Theorem' is defined as follows. A proof of a formula B on hypotheses $A_1, A_2, \ldots, A_n$, is a finite sequence of formulas such that each member of the sequence is an axiom or is one of the hypotheses or follows by (I) from preceding members of the sequence, and such that the last member of the sequence is B. Any formula for which there exists a proof without hypotheses is a theorem. To assert that there is a proof of B on hypotheses $A_1, A_2, \ldots, A_n$ we write $A_1, A_2, \ldots, A_n \vdash B$, and to assert that there is a proof of B without hypotheses we write $\vdash B$.

All the principles of section 2 are theorems of the calculus just constructed. We will not show this in detail but will instead state some metatheorems by means of which this result can easily be established.

Note first that every theorem of the propositional calculus is a theorem of this system and that the results of making proper substitutions of w.f.f. of the calculus of causal propositions for the variables of the theorems of the propositional calculus are likewise theorems of the system. Hence the machinery of the propositional calculus can be employed in proving the principles of section 2. Note also that the Deduction Theorem holds for the calculus of causal propositions.[1]

In addition, the following metatheorems or derived rules of inference hold for the calculus of causal propositions.

(IV) If $A_1, A_2, \ldots, A_n \vdash B$, then

$$\boxed{c}A_1, \boxed{c}A_2, \ldots, \boxed{c}A_n \vdash \boxed{c}B.$$

Proof:[2] Let $B_1, B_2, \ldots, B_n$ (where B_n is B) be the given proof of B on the given hypotheses. To make the desired proof operate on the given proof as follows:

(*a*) Replace each hypothesis A_i by $\boxed{c}A_i$;

(*b*) Replace each axiom C by $\Box C$, $\Box C \supset \boxed{c}C$ and $\boxed{c}C$;

(*c*) Replace any B_i that is neither an axiom nor an hypothesis by $\boxed{c}(B_j \supset B_i) \supset . \boxed{c}B_j \supset \boxed{c}B_i$, $\boxed{c}B_j \supset \boxed{c}B_i$, and $\boxed{c}B_i$; where $B_j \supset B_i$ and B_j are the premises from which B_i was inferred in the given proof.

The resulting sequence is the desired proof by virtue of (I), (III), (6), and (10).

(V) If $A_1, A_2, \ldots, A_n \vdash B$, then

$$(\alpha)A_1, (\alpha)A_2, \ldots, (\alpha)A_n \vdash (\alpha)B.$$

Proof: Similar to that of (IV) but using (I), (II) and (8).

(VI) If $\vdash A \supset B$, then $\vdash \boxed{c}A \supset \boxed{c}B$.

Proof: By means of (IV), (I), and (10).

(VII) If $\vdash A \supset B$, then $\vdash (\alpha)A \supset (\alpha)B$.

Proof: Similar to that of (VI), but using (V) and (8) with (I).

University of Michigan.

[1] *Cf.* Church, *op. cit.*, sections 2.4 and 1.9 respectively.

[2] This proof is based on one of Fitch's, *op. cit.*, p. 117.

IX.—MAXIMS

By Ernest Gellner

It may be true that the traditional philosophy of mind went wrong in treating states of mind as occurrences and in treating the relation between them and actions as causal. It would however be a mistake to assume that therefore there is no use for notions such as "rule of action", or Kant's "maxims", or that such rules and their relation to action cannot be defined in a manner not involving the above two interpretations. A rough account of something Kant said would be that events happen in accordance with laws, and actions in accordance with conceptions of laws. He was then saying something important about the logic of "action", and part of my aim is to interpret his and similar statements in a manner which will be compatible with the results of contemporary preoccupations with the way certain psychological usages mislead us, and yet enable rules or maxims to do some of the work for which they were invented.

There is perhaps a not too unnatural sense in which all actions can be said to be applications of rules, and, from the motives just indicated, I am trying to discover or invent such a sense.

Consider a schoolmaster watching a pupil perform some action, and commenting to a colleague, "He knows a lot about history, quadratic equations, and . . . . ". A celestial observer with unlimited time and sources of information might completely enumerate everything the observed pupil knows, believes, likes. Completeness could perhaps only be had at the cost of some reduplication because convictions and preferences can be described at different levels of abstractness; this doesn't matter for the present argument, and will be seen to be relevant later. One further point: expressions like "Tommy knows that . . . " and "Tommy believes that . . . " must for the purposes which will emerge shortly be lumped together, because although they imply wholly opposed things about the reporter's agreement with Tommy, yet their implications *about Tommy* are largely similar.

Having made a complete inventory of what Tommy accepts, (as true, preferable, etc.) the items of which we can describe as "the ideas he has", whilst Tommy is innocently performing some action, the celestial observer might then split up the items into two groups. One group will consist of the ideas

relevant to the action which Tommy is just performing, of those which, if Tommy did not accept them or if he accepted some alternative, would lead to a modification, or abstention from, the action. The other group would consist of those which make no such difference. Both lists would include items such as "He knows London is south of Edinburgh", and such as "He considers travel an agreeable experience". Such heterogeneous items do indeed mix happily in biographical works.

The rule on which the action is based or of which it is the application, is the list of *relevant* biographical features or facts describable as "ideas" in the sense suggested above. This definition of "rule of action" helps to do what some writers wanted rules to do, *i.e.* provide criteria for distinguishing actions from mere events. Biographical facts about a person's ideas are not relevant to mere events in the way in which they are relevant to actions. To omit the pejorative "mere" and ennoble an event into the category of actions entails that it has an intimate connexion with biographical facts of the believing/thinking/preferring type. This continues to be of interest even if we give a dispositional account of some or all of these biographical facts.

Consider some consequences of this. The rule on which the person is acting being open, *i.e.* applicable to other situations—which it will not always be in effect if the list of ideas contains demonstratives or very specific descriptions or any other *localising* expressions, so to speak—the person must at the time of action be committed to acting similarly in analogous situations. If an analogous situation turns up and he does not act similarly, this shows that his character has changed, that he has changed his convictions, attitudes or preferences. All this may look like aprioristic psychology; I am in fact only making explicit what is implied in the above definition of "rule" and offering a suggestion towards the definition of "change of character".

All that is claimed so far is that there is an intimate relation between any action and a subset of a person's mental characteristics such as convictions and preferences and that there is some point in calling this subset "the rule" of which the action was an application. Some but not all of these features will be cognitive. The force of this qualifying epithet is that a biographer reporting one of the cognitive ideas can always describe it as true or false. It is worth noting that the biographer may do this explicitly by using the words "true" or "false", or on the other hand implicitly by choosing the appropriate verb introducing the idea. By saying that Tommy knows, instead of *believes*, he is implying that the conviction to be described is a true

one. The division of ideas into those which are characterisable as true or false and those which are not is important for my argument.

Let us return to the question concerning how the subset of relevant ideas which make up a rule are selected from a person's total set. Suppose the person in question has a dossier in which his ideas are listed, one per sentence. Those ideas which are described by sentences which could cease to be true of him without this leading to an appreciable modification by him of the action as a physical event do not enter into its rule; the others do. This dependence of action on belief is indeed logical and not causal, for a part, though not the whole, of the evidence for what a person believes is whether, as we say, he acts on it. But there is a causal element present, or rather there is an indication of where to look for a causal connection, *viz.* between the events constituting the agent's acquaintance with the evidence for the truth of the cognitive ideas on the one hand, action as a physical event on the other. Not all ideas need be such as to allow of evidence, and indeed any set constituting a rule must, as I shall attempt to show, contain some which are not; some or most, however, will be. There is another slight complication: "evidence" tends to mean "good evidence", whereas the facts leading to conversion to a belief and hence to an action may be inadequate. I propose, however, to mean by "evidence" in this context whatever causes conversion and can significantly be quoted by the person concerned as justification of it, irrespective of whether, by some standard, it is a good justification. Noticing evidence for a change of belief causes or helps to cause actions; the point of time, which can frequently be ascertained, at which as it were the evidence becomes effective, *i.e.* the time at which the evidence has sunk in and after which the agent would act otherwise than he would have acted previously,—that point of time dates "conversion". Conversion is often veridically recorded by verbal behaviour, though of course to be converted and to say that one is are not the same thing. One does not feel inclined to say that the verbal behaviour expressing conversion ("since yesterday I have believed that . . . ") is an intermediate causal link between noticing the evidence and the action but rather that it, like the action, is a consequence of noticing the evidence. I am not quite clear whether this disinclination is logically or empirically founded; it may be that we use language so as never to allow this kind of causal efficacy *merely* to the uttering or hearing of words, and shift the responsibility on to the "really meaning" or "really understanding" of them, which puzzlingly turns out on reflection to be

just the uttering or hearing plus actually acting in accordance, *i.e.* something not eligible for the status of cause. On the other hand it may be also just an empirical prejudice; philosophers have warned us against the belief in the magical power of words, but there also exists at least as strong a tendency to believe in their magical impotence. One of the reasons why psycho-analytic theories surprise one is that they look as if they entail that merely *saying* something, in certain circumstances, has far-reaching effects.

The above analysis throws some light, I think, on why psychologising *a priori* and, for that matter, apriorism with regard to history, has an appeal that survives more tenaciously than similar temptations do in the theory of the natural sciences. A Humean theory of causation to the effect that it is useless to investigate either of the related terms alone for purposes of finding evidence for a causal connexion tends somehow to be more acceptable with regard to nature than with regard to humanity. There is a suspicion or hope that human behaviour "makes sense", a notion perhaps similar to whatever some philosophers meant when they said that the universe was "intelligible". One feels inclined to say that it is somehow an accident if nature makes sense—falls into a coherent pattern, but that it is surprising if a human being does not: as if there were an *a priori* expectation of order with regard to the latter but not with regard to the former. In a sense, I think, this belief in the presence of the *a priori* in our psychological statements is correct, though not the diagnosis with regard to how and where it arises. It arises at two points: in the connexion between evidence and convictions or ideas, *and* between the latter and action. Supposing convictions, ideas, preferences *were* events and *did* function as effects and causes, it would indeed go against the grain to say that they were such à la Hume, *i.e.* for no intrinsic reason. The truth is probably that although indeed their relations to evidence and action are not contingent, they are not causal either.

This, I think, partly accounts for the ghost in the machine, himself rather machine-like, being composed of elements with rather surprising logical properties, *i.e.* elements which though apparently discrete yet manage to have necessary relations with each other. The attack on the ghost in the machine may dispose of these elements, but on the other hand leaves one, rightly I think, with a sense of something important omitted, of an excessive assimilation of statements about intelligent human performances to successful performances in general, irrespective of whether the agent or agency is conscious or not. It seems to

omit something which Kant's statement about action being in accordance with conceptions of laws and events simply in accordance with laws brings out. My discussion of the relation between evidence, belief, rule and action is an attempt towards a further clarification of this.

As already stated, the temptation to believe in *a priori* discoverable psychological connexions comes in at two points: with regard to the relation between a state of mind and an action (*e.g.* " it was because he was angry with him that he hit him ") and between evidence and the state of mind (*e.g.* " because he saw a card up his sleeve he was angry "). Neither connexion looks contingent but somehow rational or appropriate; at the same time with regard to both counter-examples are to be expected and occur, so that if someone wishes to interpret these connexions as causal and yet somehow *a priori* knowable, he is forced to put them into a new category of either *a priori tendency* statements, or *a priori* correct as opposed to actual connexions, a kind of correct standard route from which actual tracks can deviate.

Ideas face as it were two ways; on the one hand they are dispositions to act in a certain way, yet on the other *many* of them are cognitive, *i.e.* are about something and it makes sense to talk of them being true or false and being valid or not in virtue of some evidence. A person's belief can be rational or otherwise, in whatever sense a theory is or is not rational given some evidence. A part of what we mean by calling an action rational is that the rule on which it is based is composed of beliefs which are rational in the above straightforward sense; or rather, that those of the components of the rule to which evidence is at all relevant are rational in that sense. This should bring out how very misleading it is to say that " he hit him because he was angry " is like " the glass broke because it was brittle ". You can alter a person's anger by conveying information to him; but *talking to glass notoriously makes no difference*, unless you believe in animistic theories, which consist precisely of treating events as actions, or believing them to be based on ideas and mental states which in turn are susceptible of being changed by the presentation of evidence. Though it may be true that ideas and states of mind are not occurrences, it is extremely important to note those of their features which differentiate them from things such as that of brittleness. These features are: the fact that some of them are true or false and dependent on evidence, and that all of them can be verbally quoted by the agent as justifications of his action, and that events, *i.e.* actions, can

be influenced by the presentation of evidence or arguments relevant to the ideas involved in the rule. Admittedly one discovers about one's own dispositions in a manner similar, in a very general way, to the discovery of the brittleness of glass; but glass does not tell us of its own brittleness in self-justification, and much of importance follows from that. The causal connexion between the noticing of evidence and action is part of what we mean by such psychological terms as "conversion" and "conviction". These causal connexions tend to make sense in a certain way, in that the evidence, with an intelligent person, modifies the behaviour so as to make it more effective in achieving some aim, this more effective behaviour being something dependent on that piece of evidence. One part of the anatomy of the ghost then arises thus: by (*a*) adding superfluous links in the causal chain between seeing-of-evidence and action, and (*b*) then inventing a kind of plasma in which these extra causal processes take place. This tendency probably springs in part also from a dislike of allowing inter-temporal, discontinuous psychological causation.

Rationalistically inclined moralists, noticing that states of mind and ideas are quoted in justification of actions and also that some at any rate of the factors involved in such explanations as it were embody propositions, *i.e.* are true or false, were tempted to believe that what was meant by "wrongness" or at any rate what defined the same class of actions was the kind of irrationality an action can have in virtue of the beliefs on which it is based being either false or even incompatible with each other. This would have had the merit of answering the "Why be moral?" query, for no one sets out to be illogical. But before such a theory could be plausible it would have to be shown that such a criterion does in fact define a class at least vaguely similar to the intuitively selected class of wrong actions. What however happens is that if the criterion of rightness is formulated as requiring that all the mental states or ideas *susceptible* of being rational in the way in which theories are, should be so, the criterion becomes too liberal and defines not right actions but all actions performed by intelligent and well informed agents. If on the other hand it is formulated as requiring that *all* the factors on which it is based should be rational in that sense, it becomes too strict and *no* action *can* satisfy it, as Hume has shown. It is interesting to see why Hume was able to show this. The reason is that explanations or justifications of actions in terms of motives, desires, intentions, beliefs . . . , or rather in terms of the complete set of the relevant ideas and states

of mind (= the rule, similar to Kant's " maxim ") have much in common with explanations in terms of sufficient causal condition —except, of course, that they are not causal. A correct account of a person's maxims or rules of behaviour is one from which we can infer to the action being attempted in certain circumstances, —that is just a part of what we happen to mean by a correct account in such a context, and that is why we can say that a man is committed to acting similarly unless his relevant ideas change. But it follows from the fact that we want explanations of actions in terms of motives, rules of action, maxims, to be sufficient explanations that any list of ideas relevant to a certain action cannot consist *only* of the cognitive ones, *i.e.* of those characterisable as true or false alone. The reason for this is that we have evidence for what a person believes other than the evidence implicit in that one action: his verbal behaviour, other actions, or the kind of data on which his beliefs are based plus knowledge of how he habitually makes up his mind. Given that, it follows that it is possible, what in fact often happens, that the identical set of beliefs, judged by evidence prior to the action, does nevertheless not lead to the same action. (If, as a matter of fact, identical convictions always *did* lead to similar action, neither Hume nor anyone else might perhaps ever have noticed that " reason is inert "; it would still be true.) In order then to retain ideas or motives accounts as *sufficient* explanations, we must supplement the story about the convictions by an addition about decisions, preferences or feelings. Whatever other functions these expressions have, one they have in common is that of always being usable for supplementing the story about beliefs, thus making the explanation by motives a sufficient one, one from which in the given circumstances that action and no other would follow. They are of course peculiarly suited for this job because, *not being cognitive, i.e.* not being tied to anything, they can never be shown to be impossible, false or as perhaps even unplausible in any situation and thus can always without contradiction be invoked to complete the explanation. A man may, in full knowledge of the facts, prefer a course of action leading to the destruction of the world in order to save his little finger; the " preference " here is not necessarily the name of some ghostly event, but is a quite legitimate addition to his inventory of ideas, legitimate and necessary if we are to explain his action in terms of motives in the way in which we habitually do so. Explanations by motives, ideas, mental factors generally are sufficient explanations and beliefs are some of the relevant factors; but identical *beliefs* may and do lead to differing

actions, and hence it follows if indeed these explanations are to be sufficient, that other, non-cognitive, factors such as decisions, preferences, feelings must be present to account for the action. In a sense these states of mind *can* be invented rather like equation-balancing particles. All this is only a restatement of Hume's main proof of reason being a slave of the passions. But this restatement is useful in showing where the Emotivist and the Existentialist go wrong; for neither of them really wants to say that moral actions, judgments, policies or attitudes necessarily have anything to do with a process or activity called emotion or decision respectively. What both *have* noticed is that those ideas which are classifiable as true or false and for the truth or falsity of which accepted criteria exist cannot by themselves constitute sufficient explanations; and they rightly wish to classify the necessary additional factors in terms of the method of validating their truth or appropriateness. But the point about these ideas is that, although any particular one (preference, choice, feeling) is always selected from a number, often a large number, of possible and incompatible alternatives, (in which they resemble the cognitive factors) yet there is no method of singling out or justifying the choice of that one from the others, (in which they differ from the cognitive factors). And it is just this which is being brought out when this lack of method of validation is called validation by feeling or "decision"; for the force of saying that something is a matter of feeling or decision is really that something has been chosen from a number of alternatives but that *no* justification can be offered for the choice. Nothing singles it out in the way an *experimentum crucis* singles out the tenable theory or in the way a sign post singles out the right road.

By a rule of action I mean then the list of the ideas, in this sense, some cognitive, (*e.g.* beliefs that . . .), some not so (*e.g.* preferences for . . . over against . . .) which justify an action and from which one can infer to an action being attempted in a certain kind of situation, the kind of situation perhaps being itself specified in the rule. The examples of maxims Kant gives include a specification of the conditions under which the maxim becomes operative.

I may, incidentally, seem to be over-stressing the possiblity of inference from a rule to attempted action; in practice, we use rules to justify and explain more often perhaps than to infer and predict. But the possibility of explaining an action by the rule presupposes that the former can be inferred from the latter, that, as suggested above, the rule is a sufficient condition of the

action or at least the attempt, given certain circumstances. Inference matters less in practice because we seldom have much evidence other than his actual action for somebody having a maxim.

Still, we can predict fairly well in the case of persons of strong character, that description being used for people whose rules of action are fairly rigid and effective and can consequently be reliably identified in advance of action. It might be objected that the behaviour of people of weak character is also predictable; indeed it is, but there is an important difference. We can construct the rule in accordance with which the person surrendering to a temptation is acting. Still, if he is really "surrendering to temptation", or "not in full control of himself", it will be useless to present evidence or arguments undermining some part of that rule, for despite intellectual agreement behaviour will not be modified—this being entailed by the meaning of "surrendering to temptation". In these cases, the constructed rule is analogous to descriptive scientific laws governing *events* whereas *actions* are performed in accordance with the *conceptions* of laws, *i.e.* in accordance with rules proper. My point is that the force of "conception" in the preceeding sentence need not be some act of contemplation but must be, at least, the kind of sensitivity to evidence and argument described above. The reason why psychoanalytic theories present an irrationalistic and deterministic picture of man is partly at any rate that they suggest that the latter of the above two ways of behaving obtains more frequently than we thought, that many of our behaviour-patterns which we thought based on good reasons were in fact independent of them. A detailed consideration of the kind of evidence considered sufficient to unmask a reason as a rationalisation would I think bear out my statements about the logical relation of rules and actions.

There is here an apparent circularity: we allow a dispositional analysis of ideas and states of mind and yet wish to treat it as something independent of the action for which they account. But this circularity is merely apparent; for although the evidence for ideas and states of mind may be behaviour, including verbal behaviour, yet the presence of an idea is not necessarily or even frequently validated by any single action. For instance: we may know what somebody believes from what he told us, from the fact that we know that he has a great respect for evidence and that conclusive evidence for the belief in question was at his disposal; if subsequently he acts *as if* he did *not* believe it, we don't necessarily say "he did not believe . . ." but may say "he did believe that . . . , but he was swayed by an impulse"

or something of the kind. No general rules can of course be given for just when verbal behaviour is weightier evidence than action ; in actual situations we usually know.

The need to give an acceptable account of rules of action, " maxims ", " states of mind behind actions " springs from the importance of the purposes which require these notions, which moreover do not disappear with the realisation that action involves not so much " knowing that " as " knowing how ". Ethical classifications of actions notoriously have to consider actions not merely as events but in terms of the rules behind them if they are not to be paradoxical, which moral theories stated only in terms referring to the physical aspect of actions, *e.g.* pure consequence-ethics, tend to be.

My aim is, so to speak, to show the compatibility of the psychological scaffolding of the *Grundlegung zur Metaphysik der Sitten* with *The Concept of Mind*. I suggest that Kant did not interpret in any objectionable way the psychological works built up to expound his ethics. I base this assertion on evidence such as his insistence on the presence of self-deception, *i.e.* lack of necessary correspondence between the actual rule of action and the one asserted autobiographically, and on the irrelevance of whether the agent is clever enough to *formulate* the rule at all. Admittedly such a case would also depend on showing that his preoccupation with determinism did not arise from mythical interpretations of mind.

But it is not only for moralists classifications that we need something like the rule terminology ; ordinary classifications require it as much. Many action words stand for groups of possible ways of behaving such that some members of the group will have little *physical* similarity with each other. Their inclusion in a common group is due mainly to the similarity of their rule or the " thought behind the act ".

Apart from the uses it already has, such a terminology may well turn out to be indispensable for the analysis of notions such as that of unconscious motivation, which might amount to the presence of the rule plus a disinclination to formulate it to oneself or others, and a disregard of its presence as evidence for the falsity of some beliefs about oneself. But this disinclination and tendency to disregard themselves constitute a second order rule, and the logic of " unconscious " requires that this rule of expression be equally unconscious. This, of course, sets us off on an endless regress, but there is nothing vicious in this. The regress is analogous to the one occurring in certain old definitions of " consciousness " as entailing that the conscious

person not only knows, but also knows that he knows, knows that he knows that . . . Either regress only becomes vicious if interpreted as requiring an extra entity or process for each recurrence of the verb " know ", or the phrase " disinclined to avow and consider as evidence ", respectively. What is roughly entailed is that, in one case, the agent should be capable of giving the correct answer however often the recurrent question is repeated, and that in the other case he should not be capable of giving it. An action inspired by an unconscious motive might be described in this terminology as an action based on a rule wholly impervious to information and persuasion presented in the ordinary manner, and to that extent similar to a causal mechanism as opposed to an intelligent response, but sensitive to information presented in a certain way, i.e. with the help of psycho-analysis.

Finally it is important to point out that any event occurring in a person's biography which one wishes to classify as an action can usually be explained by a number of different rules, or sets of relevant considerations, differing in degree of abstractness, though mutually compatible. Thus the same process can be described as the action of tightening a bolt, assembling a vehicle, earning a living, or helping the export drive, though, to repeat the premiss, there are *not* five or more processes. The point of having different " actions " is that different sets of considerations are relevant on deciding that the various features, of differing abstractness, should be what they are. Normally, when asking what a person does, and why, we tend to be interested only in some one feature and not in the whole number. There could perhaps be a clumsy sense of " rule " covering them all ; a more useful sense is the limited one which groups only the considerations relevant with regard to some one feature. To speak of an action is to speak of a physical process, some aspect of which is being sensibly interpreted in terms of the considerations leading up to it, the criteria of suitableness for such an interpretation being, as suggested, the possibility of avowal and influence through information. It follows that any event interpreted as an action with regard to any one feature will be liable to other interpretations with regard to other features. But : for reasons connected with the logic of explanations by motives, only one interpretation can be correct with regard to any one feature. It is likely moreover that most if not all events occurring in biographies will be interpretable as actions with regard to some features and not so interpretable at all with regard to others.

London School of Economics.

X.—A PROBLEM OF LEWIS CARROLL

By L. J. Russell

In *Statement and Inference* (p. 638) Cook Wilson gives a deductive problem of Lewis Carroll, one of a set intended to form part of the Advanced Logic which never appeared. I have not seen this problem in any of Carroll's works. Cook Wilson says that it can be solved, using his own methods, in " a very short time ", but he does not say what his own method was, nor does he give a solution. Doubtless many people have solved this for themselves, but it may be of interest to some readers to see it set out. The solution seems to me worth a brief consideration, as it brings out (i) the elementary nature of the processes involved, (ii) the importance, in a deductive problem of this sort, of judicious selection from among the data of the particular items needed at each step, (iii) the need for not eliminating from the consequences all items which could be eliminated, but preserving some of them for future use.

There are twelve premises, and a conclusion which is to be deduced from them. I give the premises in abbreviated form, transforming Carroll's universal propositions into hypotheticals (propositional functions), and using " $\rightarrow$ " as a symbol for *if . . . then.* Alongside of each premise I give a still more abbreviated form, using symbols whose meanings will be obvious. The negation of A I express by " $\bar{A}$ ", and so on. I number the premises for convenience of reference.

Active old Jew	$\rightarrow$ healthy	(1)	AOJ	$\rightarrow$ H
Indolent magistrate	$\rightarrow$ unpopular	(2)	$\bar{A}$M	$\rightarrow \bar{P}$
Rich snuff-taker	$\rightarrow$ unhealthy	(3)	RS	$\rightarrow \bar{H}$
Sarcastic magistrate	$\rightarrow$ Jew	(4)	VM	$\rightarrow$ J
Young snuff-taker	$\rightarrow$ pale	(5)	$\bar{O}$S	$\rightarrow$ Q
Rich old man, unhealthy	$\rightarrow$ sarcastic	(6)	RO$\bar{H}$	$\rightarrow$ V
Magistrate, not poet	$\rightarrow$ studious	(7)	M$\bar{W}$	$\rightarrow$ X
Rosy magistrate	$\rightarrow$ talented	(8)	QM	$\rightarrow$ T
Talented popular student	$\rightarrow$ rich	(9)	TPX	$\rightarrow$ R
Pale snuff-taker	$\rightarrow$ unpopular	(10)	QS	$\rightarrow \bar{P}$
Unpopular magistrate	$\rightarrow$ not snuff-taker	(11)	$\bar{P}$M	$\rightarrow$ S
Talented poet, active	$\rightarrow$ rich	(12)	TWA	$\rightarrow$ R
To prove magistrate	$\rightarrow$ not snuff-taker	(13)	M	$\rightarrow \bar{S}$.

Since we are asked to prove $M \to S$, consider the premises containing M. These are (2), (4), (7), (8) and (11). Since (11) gives $\overline{P}M \to \overline{S}$, all we need to show is that $PM \to \overline{S}$.

Each premise can be expressed in a variety of ways: *e.g.* $\overline{A}M \to \overline{P}$ can be expressed $PM \to A$; $VM \to J$ can be expressed $M \to J + \overline{V}$, where " + " is an abbreviation for " or ".

The most convenient way of expressing all the information we want for our purposes results from combining (2), (4), (7) and (8) in the form

$$PM \to PA\,(J + \overline{V})\,(X + W)\,(T + Q),*$$

keeping P on the Right Hand Side for future use. M is not needed on the R.H.S. because there are no remaining premises including M. P on the other hand occurs in (9) and (10).

One way of developing would be to "multiply" out the R.H.S. and consider each complex term such as *e.g.* PAJXT, to see whether one could show it $\to \overline{S}$. But there are short cuts. *E.g.* $PQ \to \overline{S}$ (10) suggests

$$P\,(T + Q) \to PT + PQ \to PT + S,$$

and so our expression becomes

$$\begin{aligned} PM &\to PA\,(J + \overline{V})\,(X + W)\,(PT + \overline{S}) \\ &\to PAT\,(J + V)\,(X + W) + \overline{S} \end{aligned}$$

neglecting the other factors in the term containing $\overline{S}$.
Again, $TPX \to R$ (9) and $TWA \to R$ (12) so that

$$PAT\,(X + W) \to R$$

while $$R \to \overline{H} + \overline{S}\ (3).$$

Whence $$\begin{aligned} PM &\to PATR\,(J + \overline{V})\,(\overline{H} + \overline{S}) + \overline{S} \\ &\to PATR\overline{H}\,(J + \overline{V}) + \overline{S} \end{aligned}$$

leaving R in because it will be useful for the next step.
But $A\overline{H}J \to \overline{O}$ (1) and $R\overline{H}\overline{V} \to \overline{O}$ (6) so that

$$PAR\overline{H}\,(J + \overline{V}) \to \overline{O}$$

while $$\overline{O} \to Q + \overline{S}\ (5).$$

Whence $$PM \to P\,(Q + \overline{S}) + \overline{S}$$

leaving P on the R.H.S. for the sake of the next step.

I.e. $$\begin{aligned} PM &\to PQ + S \\ &\to \overline{S} \text{ since } PQ \to \overline{S}\ (10) \end{aligned}$$

* To be read, If P and M, then P and A and (J or not V) and (X or W) etc.

which completes the proof that

$$M \rightarrow \dot{S}.$$

It should be noted that this kind of problem is not confined to problems about unhealthy old men. The letters A, O, J, etc. could just as well symbolise propositions, and the premises could just as well be expressed as pure disjunctions, or again as sets of incompatible propositions. *E.g.* given the disjunctions

$$A \text{ or } \overline{O} \text{ or } \overline{J} \text{ or } H \quad (1)$$
$$A \text{ or } \overline{M} \text{ or } \overline{P} \quad (2)$$
$$\overline{R} \text{ or } \overline{S} \text{ or } \overline{H} \quad (3)$$
$$\text{etc.}$$

we might be asked to deduce the disjunction $\overline{M}$ or $\overline{S}$ (13).

Or again giving the following sets of incompatible propositions: $AOJ\overline{H}$; $\overline{A}MP$; etc., we might be asked to show that M is incompatible with S. There is indeed a great variety of ways in which a problem could be stated which leads to precisely the same symbolic formulation. It would be a pity to regard Carroll's problems as not worth attention because of the particular type of premise which he used.

Birmingham University.

XI.—DISCUSSIONS

DO MACHINES THINK ABOUT MACHINES THINKING?

In a recent article, Mr. A. M. Turing proposed an alternative formulation of the question, 'Can machines think?'[1] The new formulation was based upon what Mr. Turing called the 'imitation game'. The object of the game is for an interrogator, by proposing questions to the participants in the game, to determine which of the participants is a human being and which a machine. If the interrogator cannot distinguish the machine from the human player in a high percentage of the attempts, presumably machines do think.

I would like to suggest another technique which, I believe, will not only eliminate some of the problems in Mr. Turing's game, and definitively answer the question, but will provide an additional task for a group of contemporary philosophers.

Before proposing my experiment, a few comments are in order. According to Aristotle, the property which properly distinguishes man from the rest of the universe is possession and use of the faculty of reason. If one may read between the lines of certain writings on the part of some philosophers who have been termed *Therapeutic Positivists*,[2] Aristotle's distinction requires modification; man is unique by virtue of the ability to *mis*use the faculty of reason. All philosophical activity prior to approximately 1933,[3] was, according to the Therapeutic Positivist (hereafter referred to as T.P.) due to the misuse of reason. To be more explicit, philosophers reasoned incorrectly about the use of ordinary language; this resulted in the assertion of "metaphysical" sentences. Descartes, according to the T.P., was recommending a verbal change when he asserted the reality of three substances. Spinoza, with his insistence that there is but one real substance, was only recommending an alternative notation. The fact that both Descartes and Spinoza thought that they were talking of something which was not linguistic, and were hardly interested in philology, is irrelevant, for the T.P. has the insight which reveals what they were *really* doing. The role of the T.P. is to verbally exercise the perplexed (*e.g.*, Spinoza and Descartes) by showing them puzzle after paradox resulting from their assertions, until they eventually recognise that they were merely suggesting linguistic changes. This revelation thereupon causes them to desist from masking these linguistic recommendations as statements about the universe.

Now, for the experiment: Let us take one of Mr. Turing's highly complex electronic or digital computers and, for a Christmas gift,

[1] "Computing Machinery and Intelligence", MIND, Vol. LIX, Oct., 1950.
[2] B. A. Farrell, "An Appraisal of Therapeutic Positivism", MIND, Vol. LV, January, 1946, and April, 1946.
[3] M. Black, *Language and Philosophy*, Ithaca, New York, 1949, p. 146.

send it a subscription to MIND, retroactive to October, 1950.[1] This means that the first article which will become part of its "store", and so part of its experience, will be Mr. Turing's article, on the problem, 'Can Machines Think?' The machine finds the article stimulating, probably, and a thought (the term is used loosely with no intention to prejudge the issue) runs through its wiring—it is thinking about the possibility of machines thinking! Since this is the very sort of thing which led philosophy astray for so many centuries, it will not surprise us when we discover that the machine suffers a nervous breakdown. (According to Norbert Wiener, machines have breakdowns under pressure which cannot be distinguished from the nervous breakdowns of human beings.[2]) Its efficiency is greatly decreased, the answers the machine gives are paradoxical, and the engineer is worried. Presumably, the engineer can fix the machine by ordinary means. In this instance, however, the machine fails to respond to the customary electronic therapy; the engineer is forced to call in assistance. Since the machine can be regarded as a nervous organism, the nerve specialist comes to the engineer's aid. After a thorough inspection and consultation with other specialists, the conclusion is reached that the machine's difficulty is psychosomatic. The clinical psychologist is called. After a reasonable length of time, it is evident that the Rogerian non-directive technique is of no avail, and the psychoanalyst insists that he be allowed to try his hand. The psychoanalyst finds no syndrome for which he can account in his system. The machine has no oedipus complex, for it had no parents. Since it had no childhood, plunging into its past does no good. The machine has failed to respond to any of the traditional forms of psychotherapy.

Where do we turn now? To none other than the Therapeutic Positivist, or T.P. The T.P. sits down near the machine, asks it a few questions and discovers that it is perplexed about the problem 'Can machines think'. The job of the T.P. is to show the machine that it has been making "metamechanical" statements. In other words, in discussing this problem with itself, it has really only been recommending changes in its calculus, in the binary code. This revelation should make it clear to the machine that it was only tussling with a pseudo-problem, and it will thereupon desist from making "metamechanical" utterances. If the T.P. can perform this therapy upon the machine, we have, *a fortiori*, shown that the machine *does* think, since it has been able to misuse its thinking powers! This, I suggest, is the experiment crucial. We may leave it to the engineer to work out the technical elaborations.

LEONARD PINSKY.

State University of Iowa.

[1] The suggestion that the machine be given MIND as a gift is due to Professor R. Popkin.

[2] *Cybernetics; or, Control and Communication in the Animal and the Machine*, Cambridge, Massachussetts, 1949.

A NOTE ON EXISTENCE

MR. MCPHERSON ("The Existence of God", MIND, Oct. 1950), says: "We are quite happy with talk about chairs and tables, but talk about their *existence* worries us" (his italics). I agree. But I do not think the reasons for this worry are quite what Mr McPherson supposes.

First a familiar distinction. "Is it twilight now?" We've been underground all day and we want to know. This question, let us say, belongs to type A. But there is another sort of question. "What does 'twilight' mean, Daddy? Is it twilight now?" (Time 2.30 p.m. Answer "No".) We sit in the window and watch the sun setting. Time 8.30 p.m.—"Is it twilight *now*, Daddy?" 8.40 p.m.—"Is it twilight *now?*" Let us call this a type B question. We are in no doubt as to how dark it actually is, as we were when we asked the type A question; but we are not sure (when we are young and not used to the baffling standards of adults) just how dark it *has to be* for it to be twilight. No more information on how dark it *is* will help us. Second example: Some boys tried to break into Smith's house. Instead of informing the police he gave them a severe reprimand and let them go. Did he do his duty? This is a moral issue, and again belongs to type A. But the following question is different. Smith heard what he took to be a cry of acute pain from an invalid at 4 a.m. He rushed out to summon the nearest doctor. The cry was in fact caused by a cat outside, and the doctor's sleep was interrupted to no purpose. Did Smith do his duty in summoning the doctor? Perhaps even here we might debate the moral issues; but, once they are agreed, this becomes a type B question. It is answerable along the lines suggested by Broad, Ross, and Carritt. Just as in the previous example, further information on how dark it is does not help, so here further insight into moral questions is similarly not relevant.

I wish to maintain that questions of the form, "Do there exist any Xs?" always belong to type A, never to type B.

Mr. McPherson's suggestion that "Chairs exist" might be said "as a way of helping to point out the use of 'exist'" is very ingenious, but will not do as it stands. He is trying to turn "Chairs exist" into the answer to a type B question. There is a more or less standard twilight, but there is no standard existence, not because the things that exist are hard to compare, like chalk and cheese, but because "exist" is not that sort of word. More strictly there neither is nor is not a standard existence. "Q. 'Why are chimaeras like golden mountains?' A. 'Because neither exist'" is obviously nonsense. Similarly it is nonsense to ask if chairs, duties, and numbers are sufficiently *alike* for "exist" to be applicable in any or

all cases. Puzzles such as whether or not it is twilight (in the type B sense) only arise where comparison with a standard is possible. There can be no analogous puzzle about whether something exists. If, with all requisite information available, we are still not sure whether or not to say it is twilight, we are supposed, in some quarters, to make "linguistic proposals" on the matter. This may be all right in the case of "twilight", but "existence" is not something there can be proposals about, at any rate not in the same way. The "proposal" to say that something exists is justified, oddly enough, if the thing *does* exist, and not otherwise. Any idea of resemblance to a standard is out of the question.

It is an important point of idiom that many "exist"-sentences sound odd in the raw, *e.g.* "Do duties exist?", "Do numbers exist?", and Mr. McPherson's "Do chairs exist?" Add qualifications, and oddity may disappear, *e.g.* "Did chairs exist in Virgil's time?" "Does there exist a prime number between 25 and 30?" Often instead of saying "Do Xs exist?" we prefer to say, "Are there—(or "Do there exist—) *such things as Xs?*". On Mr. McPherson's showing, "Are there such things as chairs?" should sound as odd as "Do chairs exist?", whereas, even though the answer is obvious, it seems to me to lack that distinctive oddity which frequently belongs to philosophers' questions. Again, "Do duties exist?" sounds much odder than "Are there such things as duties?". The latter might, I think, be asked by someone who doubted in general whether moral questions mattered at all. I suggest that "Are there such things as chairs?", "Are there such things as duties?", and "Are there any prime numbers between 25 and 30?" are straightforward type A questions, requests for further "information" (in a wide sense), to be referred to the common-sense observer, the moralist, and the mathematician, respectively.

We tend to suppose if we are empiricists that the common-sense observer tells us about "what really exists" in the way the moralist and the mathematician do not. Even high-faluting scientific language about electrons, superegos, etc., is regarded as justified if it can be "translated" into the language of the common-sense observer. If we are Platonists, however, the common-sense observer's story is regarded as in some way inferior, and it is the moralist and the mathematician who have insight into "what really exists". How this controversy could be settled, or how we should be wiser if we knew the answer, we are never told. From the nature of the case the participants themselves cannot be appealed to, nor would the gaining of this information add one jot to our knowledge of the common-sense world, morals, or mathematics. There are existential questions that lie within the province of the common-sense observer, and the same goes for the moralist and the mathematician; but it is a mistake to suppose that any one of the three tells us about "what really exists" any more than any other. The word "exist" can only function *within the context already given*

by this or that type of sentence. It makes no sense to use it to ascribe a special status, that of "really existing", to the alleged subject-matter of one type of sentence in contrast to that of others.

Those who defend objectivity in morals have nothing to fear except from people disagreeing at the moral level. "Do there exist any duties?" is a moral question. To say that there are does not commit us to saying that a duty either resembles or differs from a miniature chair.

The question of what words are substance-words is, of course, well worth asking. This is the philosopher's enquiry; and the specifically philosophical question, "Do chairs exist?" (as opposed to "Are there such things as chairs?") can perhaps best be interpreted as asking whether "chair"—in inverted commas—is a substance-word. The same can be asked of, *e.g.* "duty", "number", or "God". Just what it is about substance-words that leads us to suppose that they refer to a class of objects that "really exist" I do not know. Only in this sophisticated sense can "exist"-questions belong to type B. Chairs are not comparable to duties, but the *word* "chair" can quite well be compared with the *word* "duty".

The word "God" is not a substance-word, like "chair",[1] nor should "God"-sentences be verified—or even "verified"—in the same way as "chair"-sentences, as Mr. Cox seems to suppose. This does not of itself preclude the possibility of sentences about God having existential import, any more than the fact that "duty" is not a substance-word proves that there are no duties. "Does God exist?" may mean "Is there a God?", which is something like a type A question; or it may mean "Is 'God' a substance-word?", which belongs to type B. There is nothing inconsistent in answering "Yes" to the first question and "No" to the second. Indeed, this would be the view of many Christian theists, whereas it is the idea of a *substance-like* God that others find baffling.

Another illustrative case is "the unconscious". "Does the unconscious exist?" would not normally be taken as a type A question, a request for some further psychological information. What is being asked is, I think, Does "the unconscious" behave like standard substance-words? ("Chair" is certainly one of our standards here, as Mr. McPherson in effect pointed out). Freud, unlike his recent philosophical interpreters, definitely wants to show that it *does*. The unconscious is *real*, he tells us. It can *produce* effects which are *real and palpable*, and it was a great mistake on Janet's part to regard it merely as a *façon de parler*.[2] These are

[1] *Cf.* St. Thomas, *Summa Theologica* I, Q. 3, Art. 5. The following passage is particularly striking: "The Philosopher has shown (Met. iii) that being cannot be a genus, for every genus has differences distinct from its generic essence. Now no difference can exist distinct from being; for non-being cannot be a difference."

[2] See *Introductory Lectures on Psycho-Analysis*, p. 218 of Rivière's translation, and especially pp. 234-235. Italics mine.

clearly supposed to be points of resemblance to what is generally agreed to constitute the nature of substance. Freud may have been wrong; but there is little doubt as to the kind of argument he is, in effect, using.

These type B "exist"-questions can be called "metaphysical" in a perfectly reputable sense, with emphasis on the "*meta*". Where some of the traditional metaphysicians went wrong was in interpreting such questions as type A questions of a pseudo-empirical sort about the universe as a whole. Ontological status is not like social status, nor are chairs, duties, numbers, propositions, universals, sense-data, etc., the ingredients of the universe in the way fruit, currants and spices are the ingredients of a plum-pudding. Those who say "Metaphysics is impossible" are treating the word "metaphysics" as firmly wedded to this mistaken view. Many now prefer to grant her a divorce, and then readmit her into reputable society.

T. R. MILES.

University College, Bangor.

PARADOXICAL ANNOUNCEMENTS

A NEW and powerful paradox has come to light. The basis of it was produced by Mr. O'Connor in an article entitled "Pragmatic Paradoxes" (MIND, 1948). These are his words. "Consider the following case. The military commander of a certain camp announces on a Saturday evening that during the following week there will be a 'Class A blackout'; the date and time of the blackout are not prescribed because a 'Class A blackout' is defined in the announcement as an exercise which the participants cannot know is going to take place prior to 6.0 p.m. on the evening in which it occurs. It is easy to see that it follows from the announcement of this definition that the exercise cannot take place at all. It cannot take place on Saturday because if it has not occurred on one of the first six days of the week it must occur on the last. And the fact that the participants can know this violates the condition which defines it. Similarly, because it cannot take place on Saturday, it cannot take place on Friday either, because when Saturday is eliminated, Friday is the last available day and is, therefore, invalidated for the same reason as Saturday. And by similar arguments, Thursday, Wednesday, etc., back to Sunday are eliminated in turn, so that the exercise cannot take place at all." Mr. O'Connor goes on to say ". . . there is an obvious fault of definition in this case . . . it is pragmatically self-refuting . . . publication entails that the exercise can never be carried out". He later refers to this example as "rather frivolous". It is not frivolous, for a reason which has escaped him. Suppose that the Commanding Officer arranges for a blackout to take place during the period covered by the announcement. Clearly the date of its occurrence cannot be forecast from the announcement. So it will by definition be a Class-A blackout, and he will be entirely justified in his announcement that a Class-A blackout would take place during this period. Yet, in some sense "the exercise cannot take place at all". It appears that the logical gadget which established this conclusion has somehow short-circuited. I think this flavour of logic refuted by the world makes the paradox rather fascinating. The logician goes pathetically through the motions that have always worked the spell before, but somehow the monster, Reality, has missed the point and advances still. It is helpful to consider some related examples, beginning with a simpler one. Suppose a friend says to you in a knowing way "You are going to have a surprise at lunch-time to-morrow" and then tells you exactly what it is. One would be inclined to say that he has spoilt the surprise. He has falsified his first statement, by giving his grounds for making it; an interesting situation. (*Cf.* "I know something you don't know".) But he has certainly not contradicted himself in anything like the usual

sense. It is sensible enough to talk of the occurrence that is going to surprise someone unless you are speaking to that person. When you are speaking to that person, you talk either about the surprise or about the occurrence but not about both. Not because it is at all self-contradictory but because it is pointless, rather like the remark "I'll wager you can't find the roots of the equation $x^2+5x-24=0$ within thirty seconds. The roots are 3 and -8."

Consider next the inquisitor who points at two boxes on his desk numbered "1" and "2", and says "In one of those boxes is an unexpected egg. You may open them only in the order of their numbers. Which box contains the unexpected egg?" We should not be clear what he meant by an unexpected egg. To begin with it can't be completely unexpected since we know it is one of two boxes. And then we might see that if there were no egg in box number one, there would be no unexpectedness at all about its presence in box two. This makes us feel that it must be in box one; yet if it must be there how can it be unexpected? It looks as if there cannot be an unexpected egg there at all. Puzzled, we might say to the inquisitor "Look, your remark doesn't make good sense when announced to me, for I can show from it that there cannot be an unexpected egg in either box". He replies "Surely the matter is simple enough; there *is* an egg in one of the two boxes and you can't argue it away, nor have you by argument decided in which box it is. Ergo, it is an unexpected egg." And now we have him. For if this is what he means we can argue as follows. "If the egg is not in box one, we should know before opening the second lid that it was in box two, and it would not be unexpected. So the only possibility which might make you right, is that the egg is in box one. Yet since we shall expect to find the egg there, it will not be unexpected; in fact, the egg cannot be unexpected, so you are wrong. We do *not* have to show in which box the egg reposes in order to show that it is not unexpected, but only that it cannot be in either and be unexpected. We were puzzled at first because it seemed the egg had some magic property that caused it to vanish whenever we had deduced its whereabouts. But now you tell us that there is a real egg in one of the boxes and you go on wrongly to claim that it will not be expected. True, it may be expected wrongly but only if your own statement was wrong. There is something very queer about the statement "There is an unexpected egg in one box". It suggests, or can be taken to suggest one state of affairs: the presence of a special kind of egg. But we find that this isn't a special kind of egg in the way that speckled, double yolked, or Easter eggs are special. Not at all: unexpected eggs, for one thing, may be expected eggs at the same time (with respect to the conditions of some other problem and problem-solver). Yet the argument proceeds as though they were eggs with some observable peculiarity, and when (on that assumption) we have deduced their whereabouts, the prize is magically spirited away. The

argument which works so well with addled or ostrich eggs whose presence is asserted in one of the boxes, will not do for unexpected eggs (of, if you like, the assertion won't do), because neither of the possible alternatives, (1) there must be an unexpected egg in box one, or (2) there must be an unexpected egg in box two, is sensible if the unexpectedness applies to the person to whom the alternatives are presented. Since these statements are not in themselves sensible, it is not sensible to talk of ruling them out. Nor sensible to make a statement which is equivalent to a disjunction of such alternatives."

Now consider the extent to which our discussion is relevant to the blackout paradox. We must be clear that " Class-A " does *not* mean " announced on or after 6 p.m. of blackout night, until which time no-one had been certain there would be a blackout " but " which the participants *cannot* know is going to take place prior to 6 p.m. on the evening in which it occurs ". (Mr. O'Connor's words.) What is meant by " cannot know " ? I think the obvious intention is that it should mean " cannot produce a proof ", *i.e.* that a Class-A blackout should be one whose occurrence cannot be deduced from the conditions given. If one is given conditions governing the occurrence of ordinary blackouts then from these one can sometimes show that a blackout must take place on a particular night though no specific announcement has been made about that night: the blackout is then not a Class-A blackout. When it is impossible to tell beforehand that there will be a blackout, and a blackout occurs, then it is a Class-A blackout.

Notice two differences between this paradox and the case of the unexpected egg. First, one can expect to find an egg in a box without proving that it must be there; disappointment is less devastating than disproof. Secondly, statements which are equivalent to disjunctions of pointless statements are pointless, but though statements equivalent to disjunctions may be announced, and the terms of the disjunction might make pointless announcements by themselves, we cannot conclude the pointlessness of the disjunctive announcement.

It now becomes necessary to examine the nature of announcements more closely. It is sufficient to point out two importantly different classes of announcements. The first are publicly uttered statements. The second are ordainments. Discoveries are sometimes announced, and they are announcements of the first kind. Dates for performances and places for meetings are announced; these announcements are ordainments, in a sense guarantees, and are of the second kind. The confusion between these functions of an announcement is an important source of the power behind this paradox. In the sequel I shall make this distinction as between public statements and ordainments, though neither term precisely identifies the class to which it will refer. At first, however, the essential distinction is between ordainments and statements which are *not* announced.

Normally, the fulfilment of an ordainment comprises the verification of the statement made in the same words, *e.g.* the announcement " There will be a blackout next Saturday " (1) is fulfilled when a blackout occurs on Saturday next and the statement made with the same sentence is also verified. The peculiar and puzzling feature of some sentences employing the term " Class-A " is that their use as an ordainment may be improper while as a statement they are a straightforward contingent prediction. Consider the sentence " There will be a Class-A blackout next Saturday " (2). As a statement this is true or false depending on the announcements governing the occurrence of the blackout, and on whether the blackout in fact occurs next Saturday. But as an ordainment it has certain peculiarities. An ordainment typically fixes the time, place, or details of some occurrence or function. Now to ordain the Class-A property of a blackout is to guarantee that its occurrence will be arranged relative to the governing announcement, and the latter will be framed, in such a way that the precise date of the occurrence will not be announced or be deducible from the announcement made. If, while ordaining this, one also announces the precise date of the occurrence, one has made a self-refuting announcement. It is important to decide whether it can properly be said to follow from the ordainment (2) that there will be a blackout on Saturday. Now (2) guarantees the occurrence of a blackout which is to be on the one hand, not specified as to date, and on the other, specifically next Saturday. Granted the incompatibility of these qualifications when ordained, one can conclude that the blackout will be on Saturday and not Class-A or not on Saturday but Class-A. Neither is a proper conclusion : each is an interpretation which we might make in a situation where we believed the self-refuting character of the announcement to be due to a mistake. Since we can draw no proper conclusion from (2) as an ordainment it follows that if a blackout does take place on Saturday, while (2) is the governing announcement (ordainment), it will be a Class-A blackout. From which it follows that (2) as a statement will be correct.

Finally, consider the sentence " There will be a Class-A blackout next week " (3). As a statement, (3) will be true or false depending on the governing announcement and on the occurrence of a blackout next week. Take the particular case in which the governing announcement is (3). This announcement is also self-refuting, in a slightly less obvious sense than (2) ; the proof follows. Saturday is ostensibly a possible day for the Class-A blackout ordained by (3). But if the blackout is to be on Saturday, then there will be no blackout before Saturday, and on Saturday morning (3) will be equivalent to (2) which is self-refuting as we have shown. Saturday is therefore not a real possibility or else (3) is self-refuting. In general, a Class-A blackout cannot occur on the last of any sequence of nights during which it is ordained or else the governing announce-

ment will be self-refuting. The first five nights of the week now form such a sequence: at the next stage, the first four. And thus the nights of the reversed week fall one by one: falling with the last is the point of the ordainment.

Now if the governing announcement is (3), which is self-refuting, and a blackout occurs on any night of the week, the statement (3) will be verified. And if it was publicly stated, it would still be correct.

Conclusion. At first we thought the reductive proof showed a Class-A blackout to be impossible while in fact any blackout that took place was a Class-A blackout. Now we have come to see that the suicide of the announcement as an ordainment is accompanied by its salvation as a statement.

MICHAEL SCRIVEN.

Oxford University.

XII—CRITICAL NOTICE

A Study of Hegel's Logic. By G. R. G. MURE. Oxford : Clarendon Press, 1950. Pp. viii + 371. £1 10s.

L'OUVRAGE de G. R. Mure sur la *Logique* de Hegel fait suite à une *Introduction à Hegel* dont les premiers chapitres sont consacrés à Aristote. Cette confrontation de Aristote et Hegel est aussi éclairante pour apercevoir l'intention fondamentale et comme le projet de la philosophie aristotélicienne, que pour comprendre l'ensemble du système hégélien, réalisant en l'homme même cet acte pur de la pensée de la pensée, de la conscience de soi, que le philosophe antique situait en un Dieu séparé du monde et étranger à lui. L'esprit du monde est chez Hegel, en fin de compte, la conscience de soi que l'absolu a de lui-même. Tout dans le système hégélien aboutit à cette conscience de soi suprême par laquelle on pourrait définir la philosophie. G. R. Mure dit très justement que l'esprit—l'Absolu selon Hegel—est constitution de soi, que cette constitution de soi est manifestation de soi de l'esprit ; et cette manifestation de soi n'est pas un *πάρεργον* de l'esprit, mais sa fonction essentielle. L'esprit est dans l'experience humaine la révélation complète de soi-même à soi-même. " Self constitution in short here means self-manifestation, self manifestation means self-consciousness, and this alone is complete activity ". Mais cette activité extra-temporelle telle qu'elle apparait dans la *Logique ontologique* de Hegel, aboutit à une synthèse suprême, au risque de nous donner dans cet achèvement un sentiment de claustrophobie.

Pour nous permettre d'interpréter cette Logique hégélienne à la lumière de l'histoire de la philosophie, particulièrement d'Aristote et de Kant, le livre de G. R. Mure est indispensable. Son introduction sur la présupposition de cette logique—la philosophie du langage chez Hegel—et sa conclusion sur l'impossibilité où se trouve Hegel de résoudre complètement les contradictions de l'expérience humaine, impossibilité qui réserve les droits d'une dialectique plus modeste et permet de sauver un certain hégélianisme de principe, sont très remarquables ; elles encadrent un exposé objectif et sympathique de toute la dialectique de la Logique où sont présentes avec le maximum de clarté les passages les plus obscurs de cette Logique, ceux sur le faux infini ou les catégories de modalité par exemple. Mais si nous accordons cet éloge à l'ouvrage de G. R. Mure, nous constatons qu'il existe aujourd'hui d'autres possibilités d'interprétation de la dialectique hégélienne. Hegel n'est pas seulement l'auteur de la *Logique* et de *l'Encyclopédie*, il est encore celui de la *Phénoménologie de l'esprit*, son voyage de découverte, et la publication depuis 1907 des travaux de jeunesse, puis des *Philosophies de*

l'esprit d'Iéna permet d'apercevoir chez lui une *dialectique du réel*, dont la dialectique de la Logique—cet empire des ombres—n'est peut-être qu'une traduction. La clé de la dialectique hégélienne, de cette espèce d'énigme qu'elle nous présente—il y a déja longtemps qu'on a parlé du *secret de Hegel*—serait alors à chercher dans la signification de la négativité comme liée à l'opération humaine. L'homme, dit la *Phénoménologie*, est seulement son opération et cette opération—véritable négativité—est ce qui transforme la nature par le travail, hausse la conscience de soi singulière à la conscience de soi universelle. La séparation de l'essence et de l'existence confondues dans l'immédiat de la nature est l'œuvre même de l'homme, c'est à dire de l'esprit selon Hegel. Certes la négativité est déjà *en soi* présente dans la nature avec la dispersion spatiale, avec le cycle de la vie et de la mort dans les espèces, mais cette négativité n'existe pour soi que dans l'opération humaine ; c'est l'homme qui introduit l'absence et le néant dans la plénitude de l'être. Il y a donc bien une dialectique du réel—si le réel comprend l'homme—que Hegel a commencé par penser directement, par example dans le *System der Sittlichkeit* ou dans la *Real philosophie* d'Iéna. C'est cette *dialectique du réel* qui servirait de modèle à la *dialectique logique*, et c'est en l'étudiant d'abord qu'on pourrait tenter de comprendre cette dernière. Mais ces deux voies d'accès à l'hégélianisme ne sont pas contradictoires : la première, plus traditionnelle, conforme aux interprétations de l'idéalisme anglais, consiste à repenser le système hégélien à partir de *l'Encyclopédie* et des grands systèmes philosophiques du passé, elle a pour elle la forme même que Hegel a voulu donner à son système achevé, c'est celle que développe G. R. Mure dans son ouvrage ; la seconde très répandue dans la philosophie française actuelle depuis les ouvrages de J. Wahl, A. Kojeve, et nos propres travaux sur la *Phénoménologie*, est la source d'une certaine renaissance, non du système hégélien comme tel, mais d'un intérêt pour la dialectique hégélienne, il nous semble même parfois que certaines remarques de G. R. Mure sur la différence entre l'*acte* d'Aristote et l'*activité* hégélienne (Wirklichkeit), ou certains développements critiques sur les limites du système hégélien pourraient servir de transition entre ces deux interprétations de l'hégélianisme.

De toute façon une étude objective de la Logique hégélienne, à la lumière des grands systèmes du passé auxquels chaque catégorie de cette Logique de la philosophie renvoie, est une tâche indispensable, et le mérite du commentaire de G. R. Mure ne saurait sur ce point être contesté, même s'il n'éclaire pas d'une façon suffisante à notre avis la signification de la dialectique hégélienne. La comparaison entre le syllogisme aristotélicien et la triade hégélienne est très suggestive. Celui-là convient à la classification de l'histoire naturelle, au monde que conçoit Aristote, celle-ci exprime admirablement la notion de la conscience de soi qui domine tout l'hégélianisme : " Whereas the syllogism reflects a world of substance

and accident, or in its generalized forms a mere classifiable multiplicity of things, the triad is the abstract reflection of the return of spirit upon itself in selfconscious activity and the abstract symbol of that union of coherence and comprehensiveness which is the inherent demand of thought as it becomes selfconscious " ; on ne peut mieux exprimer la double exigence de la pensée hégélienne qui veut réconcilier en elle l'ideal de l'*analyse a priori* et de la *synthèse a posteriori ;* mais peut-être cette réconciliation est-elle impossible en fait : même si l'on tient compte du prodigieux effort de Hegel pour faire rentrer dans la pensée même l'*autre* de la pensée, on ne peut s'empêcher de constater avec G. R. Mure les limites de cette dialectique qui ne parvient pas à surmonter complètement la contradiction de l'empirique et de la pensée, du sensible et de l'intelligible, du contingent et du necessaire, du singulier et de l'universel. G. R. Mure a noté avec précision les différences des dialectiques de l'*être,* de l'*essence* et du *concept,* il a montré quel sens différent prenaient la contradiction et " l'Aufhebung " dans ces diverses dialectiques, et il en conclut que c'est la *Logique de l'essence* qui exprime le mieux cette contradiction irrésolue dans l'expérience humaine. Cela tient sans doute à ce que l'essence énonce, par sa séparation d'avec l'existence, la dualité ambigue qui est selon l'auteur omniprésente dans toute l'expérience humaine, et dont Hegel ne parvient jamais à se débarrasser complètement. Par contre la Logique du concept, avec son dépassement de toutes les oppositions culminant dans l'idée absolue qui est conscience de soi, énoncerait un au-delà inaccessible ; mais si cette idée absolue était interprétable à partir même de l'activité humaine, qui apparaitrait comme la source de la séparation de l'essence et de l'existence, du possible et du réel, le problème sans être pour autant résolu, pourrait peut-être se poser autrement. Il resterait en effet à se demander ce que signifie ce savoir absolu, dont la logique est la partie principale, pour l'auteur de la *Phénoménologie.* G. R. Mure, parlant de la dualité irréductible omniprésente dans l'expérience humaine, fait allusion à la *conscience malheureuse* de la *Phénoménologie,* mais la *Phénoménologie* est une introduction à la *Logique,* elle reconstitue une histoire de l'esprit, de la religion, et même une histoire de la philosophie qui doivent conduire l'homme à ce *savoir absolu* dans lequel la *conscience malheureuse,* aussi bien que la *religion* au sens strict du terme, sont dépassés. Le temps est venu dans lequel ce *savoir absolu,* intégrant les systèmes philosophiques du passé, est possible pour Hegel. C'est à ce moment là—et l'esprit dit Hegel est dans le Temps jusqu' à ce qu'il se soit élevé à cette conscience suprème de soi—que l'esprit peut élaborer cette pensée de lui-même qui dans l'histoire dépasse l'histoire. Il importerait donc de chercher à interpréter, à la lumière de cet itinéraire de la *Phénoménologie,* le sens de la *Logique* hégélienne, ou si l'on veut de la pensée philosophique. Hegel pense que sa Logique si elle est susceptible dans le détail de nombreuses corrections et améliorations, n'en constitue pas moins en principe la

vraie et la seule philosophie possible. Que fautil penser de cette prétention ? Peut-être G. R. Mure qui insiste tellement sur les relations d'Aristote et de Hegel n'insiste pas assez sur celles de Spinoza—et aussi de Leibnitz—et de Hegel. Il indique bien à propos de la catégorie de l'Absolu, dans la Logique de l'Essence, que c'est la pensée de Spinoza que Hegel interprète et critique, il montre comment pour Hegel le défaut de Spinoza est de ne pas avoir saisi le mode comme expression véritable de l'Absolu, en en faisant une négation de la négation. Cette critique de Spinoza, puis de l'harmonie préétablie de Leibnitz, nous ramène à la pensée fondamentale de Hegel, selon laquelle l'*Absolu n'est pas seulement substance, mais sujet ;* et ce sujet se manifeste dans la négativité absolue du mode et non dans l'infinité en soi de la substance. Mais le mode c'est la conscience de soi humaine. L'opposition de Hegel à Spinoza est ici significative, et c'est peut-être en l'approfondissant qu'on pourrait le mieux comprendre toute la signification de la philosophie hégélienne, de ce qu'elle prétend être, cette révélation de l'Absolu, par et dans la conscience de soi humaine.

Aristote, nous dit G. R. Mure, a défini l'esprit comme activité, non *par ce qui a* activité, *ce qui* est actif, mais par l'acte même, tandis que chez Platon les formes sont encore séparées du monde qui les réalise. Cependant Aristote en reste trop encore au point de vue du spectateur extérieur, il était réservé à Hegel de conduire à son terme cette notion de l'activité, de faire vraiment de l'esprit le sujet qui se constitue lui-même et se réfléchit sur soi à travers toute l'expérience qui lui apparait d'abord comme étrangère. C'est pourquoi Hegel transforme l'opposition de la Puissance et de l'Acte, en celle d'une opposition de l'apparence et de la réalité, de l'esprit qui s'ignore encore lui-même et de l'esprit qui se sait. De même la substance absolue de Spinoza n'existe pas pour lui ailleurs que dans le mouvement du mode pour se dépasser lui-même, nier sa négation. Nous acceptons la plupart des analyses de G. R. Mure, et son commentaire si précis de la *Logique*. Il nous semble cependant que son interprétation de la dialectique hégélienne dans la *Logique* pourrait s'éclairer davantage encore en approfondissant ce que Hegel entend par *activité* et *négativité*, en cherchant dans les premiers travaux de Hegel la *dialectique du réel* que la *dialectique logique* tend à reproduire sur le plan de la pensée pure. Cette étude suppolsrait la compréhension exacte des rapports entre les deux grandes oeuvres de Hegel, la *Phénoménologie* et la *Logique* ; elle conduirait peut-être à une interprétation nouvelle des rapports de la Logique et de l'histoire de la philosophie.

JEAN HYPPOLITE.

Sorbonne University

XIII.—NEW BOOKS

L'Évolution de la Psychologie d'Aristote. By FRANÇOIS NUYENS. Louvain, Éditions de l'Institut Supérieur de Philosophie, 1948. Pp. 353. 21s.

THIS, the authorized translation of a work published in Dutch in 1939 under the title of *Ontwikkelingsmomenten in de Zielkunde van Aristoteles*, has now appeared in French under the guiding hand of Prof. A. Mansion, and its importance is such as to make this second appearance welcome. A confessed, though critical, disciple of Jaeger, Nuyens sets out to trace the development of Aristotle's psychological views from the *Eudemus* to the end of his career, and to do this he has combed thoroughly not simply the professed psychological works but the whole of Aristotle's writings. Nor does he stop there, for he uses the psychological passages he finds as criteria by which to date the works in which they occur, with results differing from Jaeger's in several respects. The book closes with a lengthy commentary on *De Anima* Γ, 4-5, which is perhaps one of its most valuable parts.

Nuyens distinguishes three main stages in the development of Aristotle's psychology: (1) a Platonic, represented in the *Eudemus* and less purely in the *Protrepticus* and *De Philosophia*, which point forward to (2); (2) a phase of 'mechanistic instrumentalism', in which body and soul are still conceived as distinct entities, but the former is no longer hostile to the body but its instrument, and the latter is located in the heart; this stage is represented in part of the *Parva Naturalia*, in the biological works in general (*De Generatione Animalium* apart), and in large parts of the *Metaphysics*, together with the whole of the *Eudemian* and *Nicomachean Ethics* and the *Politics*, and the physical writings in general; (3) a final stage in which soul is conceived as the form or entelechy of the body, represented in *De Anima*, part of the *Parva Naturalia*, *De Generatione Animalium* and part of the *Metaphysics*.

It will be plain that the book is rich in material and will be valuable for all future investigators. In general, however, it is most successful in dealing with the psychological and biological works; the analysis of the cruder 'instrumentalist' theories of parts of the *Parva Naturalia* and of (*e.g.*) *De Motu Animalium* and *De Partibus Animalium* is particularly striking, as also is the way the treatment of *De Anima* is reinforced by references to *De Generatione Animalium*. Here we have a contribution of permanent value, and it is when Nuyens treads outside these limits that his step becomes less secure; then his programme becomes over-ambitious, building edifices on foundations that crumble beneath the weight. A notice as short as this may perhaps be forgiven if, when due allowance has been made for the merits of the book, it concentrates on points of criticism:

(1) The *Protrepticus* and *De Philosophia* are both spoken of as advancing a marked stage beyond the psychology of the *Eudemus*, and as being approximately contemporary works, both prior to the death of Plato. The argument that Aristotle's views had changed between the *Eudemus* and the *Protrepticus* is unconvincing, since the suggestion of the soul as ideally governing the body is quite in the Platonic tradition, and the moral interests of the *Protrepticus* place it rather on one side as the first stage

in Aristotle's specifically moral psychology. (It will be seen below that on the existence of such a moral psychology I disagree with Nuyens.)

(2) Nuyens has certainly shown that not all Jaeger's arguments for placing the *De Philosophia* after Plato's death are as cogent as he thought; on the other hand, Jaeger does enough to show that such a date is psychologically probable from the point of view of Aristotle's development, and Nuyens's positive arguments for an earlier are not convincing. The *De Philosophia* marks a special development of its own, and Nuyens neglects the important discussions of Bignone (*L'Aristotele perduto e la formazione filosofica di Epicuro* (1936), Vol. I, pp. 227-61), followed more recently by Moreau (*L'âme du monde de Platon aux Stoïciens* (1939), pp. 120-39), Bidez (*Un singulier naufrage littéraire dans l'antiquité* (1943)) and Festugière (*La Révélation d'Hermès Trismégiste*, Vol. II (1949), pp. 247 ff.); these seem to show that the doctrine of the soul there was materialistic, bound up with the conception of aether as a fifth and divine element. At any rate, we are here far from the *Protrepticus*. It means also that Nuyens's division of Aristotle's development into three stages leaves out an important phase.

(3) We turn now to Nuyens's handling of the treatises, and there several criticisms may be made. The first is that Nuyens, though not always, yet tends excessively to treat whole works as single units when they may have been composed over a lengthy period. Examples are the *Historia Animalium*, *Politics* (despite qualifications) and *Nicomachean Ethics*.

(4) Nuyens uses indifferently for his purpose any and every type of treatise. But the different purposes for which they were composed need to be taken into account. For instance, though the *Topics* was certainly an early work, Nuyens's references to the psychological examples found in it, picked up from stock discussions of the day, do little to prove the fact; their force is purely illustrative of their date, when once that has been fixed on other grounds. Again, though Nuyens is right to place the *Prior* and *Posterior Analytics* in the transitional period, the latter is not as uniform as he takes it to be, and arguments as to their date can rest securely only on an examination of Aristotle's development as a logician, for guidance on which Nuyens's treatment cannot help us; we have to turn to the type of argument advanced by Sir David Ross in his recent edition. Nor, again, can Nuyens provide anything of real value for the dating of the physical treatises.

(5) He is, if anything, still less secure in his treatment of *Historia Animalium*, which he takes to be a unitary work belonging to the middle period. *Historia Animalium* is surely a work written over many years, involving lengthy research and probably the co-operation of several members of the Peripatos in the second Athenian period; Regenbogen now argues (Pauly-Wissowa-Kroll, *s.v.* Theophrastos) that Theophrastus had an important hand in the later books. Nuyens lists several passages where Aristotle compares human characteristics with animal, but, beyond the fact that these presumably belong to a post-Platonic period, it is difficult to see what they prove.

(6) The same applies to several of Nuyens's references to the *Metaphysics* (those to *Z H Θ*, and perhaps *Λ*, apart). The references, for instance, at p. 172, n. 72; p. 173, n. 74; and p. 175, n. 81 provide little for his purpose.

(7) Nuyens joins issue (p. 193) with Burnet and Jaeger, saying that it is improbable that Aristotle should have had a special moral psychology, distinct from the scientific. But he gives no real reason for so thinking, and his programme forces him to ignore Aristotle's own words at *E.N. A.*

1102a 18 ff., to lay excessive stress on *obiter dicta*, and to crowd Aristotle's development as an ethical and political thinker into an excessively narrow space. He provides no adequate reason for denying a big gap between *E.E.* and *E.N.*, and ignores Festugière's proof that the treatment of pleasure in *E.N.* K is later than that in H (*Aristote : Le plaisir* (1936): see now also Léonard, *Le bonheur chez Aristote* (1948), pp. 201-8). In fact, Nuyens's own treatment of the development of Aristotle's scientific psychology makes it clearer than before that the psychology of the moral (and political) writings cannot be fitted into the same framework.

(8) Nuyens thinks *De Anima* a homogeneous work of the end of Aristotle's career. He discusses and rejects Jaeger's arguments for holding that *Γ* is earlier than *A* and *B*, referring to an article by von Ivánka in *Rev. néo-scol. de phil.*, xxxii (1930), pp. 75-83. But to prove that *De Anima* belongs entirely to one period more is needed than the arguments of von Ivánka. Jaeger's view was, indeed, incompletely worked out: in particular, *Γ* is not all of a piece, and a more elaborate and convincing dissection of the whole work is that put forward by Langerbeck in *Gnomon*, xi (1935), pp. 416-23, analysing out of the treatise as we have it three layers, early (*spätplatonisch*), transitional and late, to the third of which belong parts of *A* and the great bulk of *B*.

(9) Nuyens says much that is valuable on the *Parva Naturalia*. He regards *De Sensu*, *De Memoria*, *De Somno*, *De Insomniis* and *De Div. per Somn.* as late, and the other works as transitional. But more recently Lulofs, who uses Nuyens's analyses, has shown that neither *De Somno* nor *De Insomniis* is internally homogeneous, and that *De Div. per Somn.* is contemporary with the earlier strata of the other two. We may refer also to Düring's suggestion that earlier drafts of *De Anima* and *De Sensu*, now lost, are referred to at *Resp.* 474b 11 and *P.A.B.* 653a 19 and 656a 28 (Düring, *Aristotle's De Partibus Animalium* (1943), pp. 11-12, 131; cf. Nuyens, p. 168; p. 170, n. 65).

(10) The effect of Nuyens's method is to attribute to Aristotle's development a rigidity and uniformity that are not to be expected in any philosopher and perhaps are not even desirable. His approach was much more tentative and even hesitant; he was never an Aristotelian, such as later generations loved to make him. This means that, while Nuyens criticizes Jaeger for failing to correlate the chronologies of the different lines of enquiry Aristotle pursued, the attempt he makes to do so cannot hope for the measure of success he claims, and while some of his conclusions mark an advance, others are simply a retrogression; so that, in view of the influence Nuyens's book has had (*cf., e.g.*, A. Nolte, *Het Godsbegrip bij Aristoteles* (1940)), a word of caution seems needed. But it is useful to have Nuyens's position stated and worked out so thoroughly, whether one finds it acceptable or not.

The concluding commentary on *De Anima Γ.* 4-5 is Averroistic in trend. It is very carefully worked out; as Nuyens realizes, it is difficult to find a consistent doctrine in these chapters, and perhaps it is a mistake to analyse them on the assumption that it is possible to do so. But Nuyens makes out as good a case for the Averroistic interpretation as can be made, and, somewhat forced though his distinction of two senses of *νοῦς* (pp. 310-12) perhaps is, it deserves close consideration.

D. A. Rees.

A History of Philosophy. Vol. II. *Mediaeval Philosophy: Augustine to Scotus.* By FREDERICK COPLESTON, S.J. London: Burns Oates and Washbourne, 1950. (The Bellarmine Series XII.) Pp. x + 614. 25s.

FATHER COPLESTON has "endeavoured to give an intelligible and coherent account of the development of mediaeval philosophy, and of the phases through which it passed" (p. 8). In this volume, the first of two meant for this purpose, he mainly expounds and discusses views held by Latin writing Christians of the first thirteen centuries: thinkers who wrote Greek, Arabic, and Hebrew are only mentioned for contributing to those views. Apart from the rather apologetic introduction and "Concluding review", a bibliography, and two abundant indexes, nearly two-thirds of the book are taken by five essays: on Aquinas (132 pages), Scotus (75), Bonaventure (52), Augustine (50), and Eriugena (23). Most of the other third is devoted to hasty summaries on such traditional topics as the patristic period, the Carolingian renaissance, the problem of universals, the translations, etc. Albert the Great and the School of Chartres (including John of Salisbury) are allotted eight pages each, as many as William of Auvergne; Abailard is dismissed in three.

In order to "discern an intelligible pattern in mediaeval philosophy" the author chooses "Thomism" (better, a kind of neo-Thomism) as his "principle of selection" (p. 9). Aquinas' philosophy is to him the highest point of a parable, whose first branch includes all previous philosophical thought: the second is decline. Aquinas gave the "charter" to philosophy, disentangling it from theology, and leading Aristotle to his due triumph: "Owing to the discovery of the complete Aristotle and his adoption by St. Thomas, so far as adoption was consistent with theological orthodoxy, St. Thomas provided the material for an independent philosophy" (p. 556). But independence "inevitably" engendered decadence.

Philosophy is conceived as something dry, abstract from life, a thing for professors holding chairs. To extract 'philosophy' from writings of people interested in life and truth regardless of possible divisions between 'philosophy' and 'theology' was the author's self-imposed difficult task. The difficulties he encountered are most apparent in the longer essays: Augustine's, Bonaventure's, Aquinas', and Scotus' 'philosophical' tenets are collected and organised from a variety of texts, most of which are 'theological'. The problems of the existence of God, creation, free will, knowledge by illumination were treated in almost each case, by the 'whole' man: it is difficult to say which share in each doctrine is due to the thinker's reason, and what derives its value or justification from faith or impulses towards a fuller life. The author's own interests lead him continuously into the province of 'theology', and his preoccupation with 'theological orthodoxy' colours the whole book. Eriugena "was in intention orthodox" (p. 131), and "if we want to call him an Hegelian before Hegel, we must remember that it is extremely unlikely that he realised what he was doing" (p. 124): he "was not a conscious pantheist who made verbal concessions to orthodoxy with his tongue in his cheek" (p. 135). "It cannot be shown that Tertullian was not a materialist", but one should "hesitate before forming the conviction that his general meaning always coincided with the words he used. . . . However, of Tertullian's theological developments and of his orthodoxy or unorthodoxy it is not our concern to speak" (pp. 24-25). Even "St. Thomas finds himself compelled" (by reason or by faith?) "to say that such truths as

the unity of God are not properly speaking articles of faith, in order that it should not appear that one is failing to give assent to articles of faith" (p. 315). Father Copleston's faith in a purely intellectual, *i.e.* purely philosophical, 'philosophy' is not very strong: "Unless one is prepared to condemn the human intellect as such or at any rate to debar it from the discovery of a true metaphysic, one must admit that the establishment of a satisfactory metaphysic is abstractly possible, even for the pagan philosopher" (p. 319): 'abstractly possible' seems to mean 'really impossible'. The problems of language and its relations to thought and things have exercised the mind of many thinkers in the twelfth and thirteenth century, and this is one of the most intelligent features of that period: probably because Fr. Copleston thinks of logic and grammar as of crystallised non-philosophical disciplines he is silent on all that activity, except for what usually goes under the heading of 'problem of universals'. 'Philosophical' errors are quasi-justified by conditions of thought: If one is a Thomist, Trinity cannot be proved, "but, if one puts oneself in the position of St. Anselm, that is, in a state of mind anterior to the clear distinction between philosophy and theology, it is easy to see how the fact that the first truth (God's existence) is demonstrable, coupled with the desire to understand all that we believe, the attempt to satisfy this desire being regarded as a duty, naturally leads to an attempt to demonstrate the second truth [Trinity] as well, and in point of fact St. Anselm speaks of demonstrating the Trinity of the Persons by 'necessary reasons'" (pp. 157-158). Many thinkers are presented as subject to a tension, due to their faith coming to loggerheads with their accepted philosophy: *e.g.* "the Pseudo-Dionysius, though an orthodox Trinitarian in intention, was so much influenced by the neo-Platonic philosophy that a tension between the two elements underlies his attempt to reconcile them and makes itself apparent in his statements" (p. 97); "the synthesis of Christianity and Aristotelianism in St. Thomas's thought was in some respects rather precarious, . . . I think it is true that he did not fully realise the latent tension, in regard to certain points, between his Christian faith and his Aristotelianism" (p. 424). In fact, this 'tension' seems to be the only cohesive element in Fr. Copleston's view of mediaeval philosophy as a whole: it also is nearly the only thing we learn about the ways in which the minds of those thinkers worked, or suffered.

After reading about the unique merits of Aquinas in regard to philosophy, and after being told that one (the greatest ?) task of philosophy is to prove that God exists, the following statements are disappointing: "The way in which St. Thomas states the proofs may perhaps cause some dissatisfaction in the reader; but it must be remembered that the Saint was primarily a theologian and that he was concerned not so much to give an exhaustive treatment of the proofs as to prove in a summary fashion the *praeambula fidei*. He, therefore, makes use of traditional proofs, which either had or seemed to have some support in Aristotle; . . . he gives a certain preference to the first, to the extent at least of calling it the *via manifestior*. However, whatever we may think of this assertion, the fundamental proof is really the third" (p. 345). And: "it might seem to be rather cavalier behaviour on his part to assume that the unmoved mover or the first cause or the necessary being is what we call God. . . . I think that we must admit that the actual phrases which St. Thomas appends to the proofs given in the *Summa Theologica* (*et hoc omnes intelligunt Deum, causam efficientem primam quam omnes Deum nominant, quod omnes dicunt Deum*) constitute, if considered in isolation, an over-hasty conclusion;

but, apart from the fact that the *Summa Theologica* is a summary (and mainly) theological text-book, these phrases should not be taken in isolation" (pp. 342-343).

The greatness of mediaeval philosophy appears, then, to lie in the re-establishment of Aristotelianism accompanied by clear thinking on the difference between philosophy and theology, reason and Christian faith. In this connexion, the temptation to clear the Church of a possible blame for 'condemning' Aristotle in the first half of the thirteenth century is understandable though not irresistible. But it is a poor defence that says, against or without all the available evidence, that the Church mistook for Aristotle's works the Plotinian *Theologia Aristotelis* and the Proclian *Liber de causis*, and for Aristotelian ideas those embodied in Averroes' writings: the *Theologia* was first known in Latin in the sixteenth century, Averroes was translated after the initial 'prohibition' of Aristotle, and probably nobody at that time—and few later on—ascribed the *Liber de causis* to Aristotle (it is false that the 'discovery' of its non-Aristotelian character was due to Aquinas).

L. Minio-Paluello.

Aquinas and Kant. The Foundations of the Modern Sciences. By Gavin Ardley. London: Longmans, Green and Co., 1950. Pp. vii + 256. Price, 18s.

The philosopher, according to Mr. Ardley, is concerned with the real order, the order which is discovered and not constituted by the human mind. The perennial philosophy is represented by Aquinas. Hence the first word in the main title of the work. The physicist on the other hand is concerned with the categorial order, the order of mental artefacts or "fictions". This view of physical science derives ultimately from Kant. Hence the last word in the main title. Further, this view of physical science is not only the chief and most valuable element in Kant's philosophy but also perfectly compatible with, and indeed complementary to, Thomism. Hence *Aquinas and Kant*. Thus the principal bogy-man of the Scholastics enters into a peaceful partnership with the Angelic Doctor. If Thomists on the one hand and physicists, sociologists and pyschologists on the other hand realised the true character of the modern sciences, the former would not think it necessary to change or modify the Aristotelian-Thomist philosophy of nature in order to accommodate it to modern science, while the latter would not imagine that their physics, sociology or psychology rules out or refutes a philosophy which is not a mental artefact but discovers and expresses the real order.

It is clear that the author attempts to treat a vast field in a comparatively short work. Unless he intended to write simply for Thomists and near-Thomists, it is perhaps unfortunate that he does not do more to substantiate his claims for Aquinas' philosophy. However, as the subtitle of the book indicates, the main topic is the nature of modern science; and his thesis on this matter constitutes the chief interest of the book. If modern science is what Mr. Ardley says it is, then it is certainly compatible with Thomism. But are the modern sciences what Mr. Ardley says they are? This is the principal question which suggested itself to my mind in reading the book. I am not specially competent to discuss the matter; but at the same time I feel convinced that Mr. Ardley's statement of the nature of the modern sciences involves an over-simplification and an exaggeration of one aspect of modern science.

The author writes clearly; and he leaves the reader in no doubt as to his interpretation of the modern sciences. He certainly realises an important element in modern physics, namely what he calls its categorial or fictional element; but I suggest that he draws too general a conclusion from the views of Eddington and kindred thinkers, and that he states his conclusion too dogmatically, without introducing desirable qualifications and distinctions. Nobody would deny, I presume, that it is possible to work out logical systems and mathematical systems which are internally self-consistent but which do not give us further information than that certain definitions and premisses have certain implications. But it does not follow that, if a scientific theory or hypothesis is applied and found to be "successful", the scientist is engaged in "thrusting laws" upon nature in such a way that he obtains *no* information about reality. (And what do the terms "reality" and "real order" mean in this connexion?) It is true, for example, that one should not "hypostatise" electrons and mesons and so on; but to say this is not the same thing as saying that the electronic theory has no relation to the real order. The theory is an hypothesis and it is, in some sense then, a "mental artefact"; it may also be likened to a sieve or to Eddington's "Procrustean bed"; but it appears to me rather cavalier to conclude from this that it is a mental artefact in the same sense that an *a priori* system of pure mathematics is a mental artefact. Again, it may well be that psychoanalysis has often proved itself a useful instrument in psychotherapy; but it is at least arguable that its practical fruitfulness, in so far as it is practically fruitful, indicates some relation to the "real order". After all, what has proved itself successful is not the mythical and philosophical bunkum associated with Freudianism but a technique based on a fairly simple assumption which is, in principle, capable of verification in some degree.

Mr. Ardley objects against a philosopher like Wittgenstein that he confines science and knowledge to the categorial order, and introduces a "violent dichotomy" between science and knowledge on the one side and life and emotion on the other. But if one confines the modern sciences to the categorial order and assigns to philosophy the privileged position of discovering the real order, does one not substitute a "violent dichotomy" of another kind?

FREDERICK C. COPLESTON.

Selected Writings of Edward Sapir in Language, Culture and Personality. Edited by D. G. MANDELBAUM. University of California Press. (London: Cambridge University Press.) 1950. Pp. xv + 617. £2 7s. 6d.

SAPIR died in 1939 at the age of 55. He was then Professor of Anthropology and Linguistics at Yale, and the title of his chair well expresses his interests and his mode of approach. While the bulk of his technical work was in linguistics, his approach to language was that of an anthropologist. Many of his papers are based on field studies of American Indian tribes. His scientific writing, apart from his book on *Language* (1921) was in the form of articles and monographs, and the most representative of these are collected in the present volume, with useful introductions by the editor, and with a complete bibliography of his prose writings and of his poems.

I have not seen any of Sapir's poems; but in his scientific work he had an extraordinarily sharp eye for the genuine concrete. Thus for example,

he was not satisfied with a purely phonetic rendering of speech sounds, which he held misrepresented the sounds heard by the users of the language, and tended to obscure the function of the sounds in use. Hence his stress on phonemes. "Language is not merely articulated sound; its significant structure is dependent upon the unconscious selection of a fixed number of 'phonetic stations' or sound units" (p. 8). He compares these 'sound units' to the notes in music.

Languages differ in their phonemic structure. They also differ in the patterning by which the phonemic materials are built into discourse, and which, as he put it, predetermines for the users of the language certain modes of observation and interpretation of what is experienced. His wide knowledge of languages made him stress the great variety of ways in which the material of experience could be handled linguistically and hence conceptually in different cultures, and the danger of using the forms of a given language as a clue to ultimate categories. Interesting illustrations of this variety are given on pages 157-159 and in the discussion of linguistic forms on pages 549 ff. Language in his view "completely interpenetrates with experience" (p. 11).

Some of the practical problems arising from this variety of linguistic patterns are indicated briefly in the paper on "The Function of an International Auxiliary Language" while the researches needed for tackling "the eventual problem of adequately symbolising thought" (p. 119) projected under the sponsorship of the International Auxiliary Language Association are partially illustrated by the paper on "Grading: A Study in Semantics". The project is described by the editor on page 5. These researches were not followed up by Sapir.

Some of the most interesting discussions from the point of view of logic and of social science are contained in Sapir's classical momograph on "Time Perspective in Aboriginal American Culture: A Study in Method" (pp. 389-462) originally published in 1916 and long out of print. Here he examines the various methods by which historical reconstruction can be made in anthropology, the last part (pp. 432 ff.) being entirely concerned with linguistic evidence. The way in which Sapir himself handled linguistic evidence for this purpose can be seen in the fascinating paper on the Navaho published in 1936 (pp. 213 ff.).

The papers in the second and third parts of the volume are very varied, but show Sapir's sureness of touch, his great suggestiveness, his concern with concrete actuality and his avoidance of abstractions, along with a steady search for generalisations about society and about the relations between culture and personality. Very few of them are technical and most of them can be read with profit by the non-specialist. One is constantly struck by his good sense, his balance, and his ability to see a long way ahead of most of his colleagues. As samples of his quality I would mention the articles on "Culture, Genuine and Spurious", on "Anthropology and Sociology", on "Cultural Anthropology and Psychiatry", and on "The Unconscious Patterning of Behaviour in Society". Sapir used the word "Psychiatry" in preference to the word "Psychology" since, as he explains in a revealing footnote on page 579, "psychiatrists themselves, in trying to understand the wherefore of aberrant behaviour have had to look far more closely into basic problems of personality structure, of symbolism, and of fundamental human relationships than have either the 'psychologists' or the various types of 'social scientists'".

The volume is a fitting memorial to an outstanding personality.

L. J. RUSSELL.

The New Science of Giambattista Vico. Translated from the third edition by THOMAS GODDARD BERGIN and MAX HAROLD FISCH. Cornell University Press (London: Geoffrey Cumberlege). 1948. Pp. xvi + 398.

VICO is one of that unlucky group of authors whose ideas are without doubt original and provocative, but who never quite succeed in attracting general attention, and so exercise, at best, a secondary and indirect influence on the development of thought. Largely incomprehensible to his own generation, and all but unknown for half a century or so after his death, it was not till the period of the German Romantics that his speculations on history found a potentially favourable public; and even then they were not much studied outside Italy until Michelet published an abridged French translation of *The New Science* in 1827. For a time it became the fashion to acknowledge Vico's genius: Coleridge, Thomas Arnold and Marx all paid tribute to him. But it was for his anticipation of the ideas of later writers, rather than for any unique contribution of his own, that he was admired; and with the decline in their reputation, he again fell into oblivion. Nor have subsequent efforts to direct attention to him, notably those of Croce, met with any considerable success: to most of us he remains little more than a name.

A large share of responsibility for Vico's reception must be assigned to his incompetence as a writer. His masterpiece, *The New Science*, in particular is a forbidding document at first view, obscure in its plan, often clumsy in expression, full of repetition and loaded with a weight of learned allusion which makes it unpalatable to any but the strongest stomachs. Yet it is, in essentials, a book with a simple and indeed an arresting theme. Vico's aims in writing it may be summed up under three heads. First, to reconstruct the early history of Rome by means of a critical examination of surviving traditions, procedures and language forms, in addition to available literary evidence. In carrying this out Vico showed himself well in advance of his contemporaries in historical method, and anticipated in many details the results of later scholarship. Secondly, and more ambitiously, to use the history of Rome as thus reconstructed as a guide to the discovery of an "ideal, eternal history" through whose stages he thought all nations must pass, and conversely to use this "ideal history" to help the empirical study of periods so far found obscure. Here it must be said that, despite his very limited knowledge of history outside the classical world, he produces results which are often suggestive and sometimes really illuminating. Lastly, to provide what he called a "rational civil theology of divine providence", a justification of God's ways to man in history. As might be expected, he is not easy to follow over this last point; his main line of argument (found also in Kant and Hegel) is to lay stress on the fact (or apparent fact) that men often achieve more than they know in history, good results emerging (as we say) providentially from actions ostensibly motivated by fear and greed.

It will be seen from this that *The New Science* has all the marks of a speculative philosophy of history of the kind practised by Hegel and (with variations) by Marx. But it would be misleading to leave the matter at that. It should be emphasised that Vico has no metaphysical axes to grind in his study of history (his epistemological views have no bearing on his results), and that his methods are, in intention at least, of an empirical nature. The "axioms" he lays down he is prepared to support

by evidence, and the success of his work rests primarily on the sharpness of his own historical insight, which led him to see striking analogies and discern general tendencies at work when others have seen nothing. The writer his book recalls most closely is Toynbee: in both we are struck by a use of analogical argument which is bold without being wild, and in both speculation is controlled by constant attention to evidence. Of the classical philosophers of history he is nearer to Comte than to Hegel; though fortunately he is without the frightful aridity which makes Comte unreadable to-day.

This is scarcely the place to point out details in Vico which deserve attention, but it may be in order, in view of contemporary philosophical interests, to mention that he has striking views on the origin and development of language, which are perhaps worth studying for their own sake (see especially pp. 124 ff. of the present translation).

Messrs. Bergin and Fisch have already put English readers of Vico in their debt by publishing, in 1944, a translation of his autobiography, prefaced by a long introduction describing his work and influence. The present volume, which is the first English translation of *The New Science*, adds to that debt enormously. So far as is humanly possible, the work is tidied up and made manageable by the addition of extra chapter headings and divisions; for convenience of reference the paragraph numbers from Nicolini's definitive Italian edition have been retained. The translation reads as smoothly as could be hoped for in the circumstances: it is obscure in places, but that is the fault of the author. The text translated is that of the third edition of 1744, issued a month or so after Vico's death. A brief bibliographical introduction, explaining the relation of this edition to its predecessor of 1730, and to the first *New Science* (virtually a separate book) of 1725, would have been useful for scholarly purposes. But light on these matters can be got from the introduction to the autobiography, which anyone intending to use the present translation would do well to have at his side.

W. H. WALSH.

Der Wiener Kreis, Der Ursprung des Neopositivismus, Ein Kapitel aus der jüngsten Philosophiegeschichte. By VICTOR KRAFT. Vienna: Springer Verlag, 1950. Pp. vi + 179. 17s.

THIS is an account of the Viennese Circle by one who took part in its first meetings when he was a *Privatdozent* and has survived to become *ordentlicher Professor* of Philosophy in Vienna when all the other members of the group are dead or scattered and the movement is already a chapter of history. The book does not pretend to make a contribution to philosophy by introducing new ideas in the guise of criticism, but it is a clear and well-ordered survey of a very interesting episode, and it will be useful to students of philosophy in our day as well as to future writers on the history of ideas. For it is impossible to take an intelligent part in the current debates of philosophers without some knowledge of the previous turns of the argument, and the minutes of recent sessions are not all easily available.

The first ten pages contain the necessary minimum of information about people and dates, and the rest of the book is devoted to consideration of the work of the Circle under various heads. Under the title '*Logismus*' Professor Kraft expounds briefly the doctrines which members of the group held about logic and mathematics and the logical analysis of language, and

then in a section called '*Empirismus*', which comprises a hundred pages, he deals with the Constitution-System of Empirical Concepts (*i.e.*, the sort of philosophy Carnap tried to do in his *Logischer Aufbau der Welt*), the Verification-Foundations of Empirical Propositions (*i.e.*, various theories of verifiability, probability and confirmation), and the Domain of the Knowable (*i.e.*, discussions about the unity of science, physicalism, and the proper view for positivists to hold about values). In connexion with these topics Professor Kraft naturally mentions the stimulus which members of the Circle received from the work of their predecessors, Mach, Russell, and Wittgenstein, but he does not attempt to trace the development of positivist or near-positivist doctrines after the break up of the Circle, except by occasional reference to Carnap's later publications. This limitation may be due partly to his difficulty in getting foreign books and periodicals, but in any case it was probably wise for him to stop where he did; the period which ended with the war has a natural unity, and it is still too early to see the developments of the last dozen years in perspective.

So far as I can judge, most of the history contained in this book is reliable; but I have noticed a couple of assertions which are not quite correct. On page 56 Prefossor Kraft says that Morris, writing in 1934, was the first to point out that 'syntax is not the whole of language', a thesis admitted and developed later by Carnap. My own recollection is that all philosophers, except those eager for a positivist version of the Athanasian creed, said something of the kind when they first read Carnap's *Logische Syntax der Sprache*; and it is on record that Tarski produced the first version of his *Wahrheitsbegriff* as early as 1933. On page 106 Professor Kraft says that Wittgenstein contributed 'the important new insight that the truth of compound sentences depends only on the truth of the simple sentences which are their parts'. If this means that Wittgenstein was the first to maintain what is sometimes called the thesis of extensionality, it is probably correct. But if it means, as later comments suggest, that Wittgenstein was the first to give a truth-functional account of conjunction, disjunction, etc., it is surely wrong. That way of explaining connectives was quite familiar to Boole, Jevons and Peirce, and it goes back ultimately to Philo of Megara.

The logical positivist movement was largely the creation of Schlick, who conceived a programme of co-operative study based on the teaching of Mach, Russell, and Wittgenstein, and engaged the interest of a group of pupils and younger colleagues. But he seems to have been free from the neurotic attitude which some of the Circle showed towards metaphysics. He liked to stress the continuity of philosophical development in opposition to the extremists like Neurath who wished to dissociate themselves from everything that had been done in the past under the name of philosophy; and he could even allow that Spinoza and Leibniz had valuable insights. Although he, like other modern positivists, held that all the problems which are commonly called philosophical can be dissolved by suitable attention to language, he did not suffer from the delusion that they can all be eliminated by one simple device such as that called translation into the formal mode of speech. His writings have therefore a freshness and a power of suggestion which is lacking in the more systematic treatises of anti-metaphysics. Unfortunately they are not as well known as they deserve to be. Professor Kraft, who obviously stands nearer to Schlick than to any other member of the group, has done a good service by explaining their importance.

At one time Schlick laid stress on the distinction between structure and content, insisting that structure alone is communicable. We have not

heard much of this distinction lately from positivists, perhaps because they think it is outmoded; but Professor Kraft shows clearly how it is relevant to discussions of the mind-body problem, and in one of the most interesting passages of his book (pp. 42-43) he argues against an attempt of Schlick himself to show that comparison of the contents of the experiences of different persons is at least logically possible. Following a suggestion of Wittgenstein, Schlick said that there was no logical absurdity in supposing he might feel a pain whenever the body of an other was affected, and concluded that it was therefore possible to attach a meaning to the statement 'I feel another man's pain'. In reasoning so he assumed that the ownership of a pain is determined logically by its association with a particular body. But Professor Kraft argues that according to this analysis of ownership A could not say he felt B's pain unless his own body were affected as well as B's, since otherwise the pain would be merely B's and not one felt by A. And he goes on to maintain that if A's body were so affected, there would be no good reason for assuming that A's pain was identical with B's, since A's curious experience would apparently be associated with two bodies whereas B's experience would be associated with one body only.

It may perhaps be objected to Professor Kraft's reasoning that in the circumstances imagined by Wittgenstein and Schlick A would regard B's body as part of his own and therefore be able to claim ownership of the pain without associating it with that part of his double body which was not also B's body. But this will not do. To say that A must call anything part of his body if he feels a pain in it is to assume that ownership of a body is determined logically by its association with a particular experience—something quite different from the assumption underlying the argument attributed to Schlick. What these speculations establish is only the conceivability of an overlap between two bodies in that sense of 'body' in which people speak of 'my body', 'his body', etc. If this occurred, the persons who owned the two bodies might very well adopt the new idiom of talking about pains in their common part as we talk of common perceptual objects like views, sounds, and smells. One might say to the other, for example, 'Did you notice that ache?' just as we say to each other 'Did you notice that horrid smell?' But so long as there were two distinct persons, capable in principle of communicating with each other, we should have to admit that the situation was substantially the same as when two persons with non-overlapping bodies communicate. The mere fact that two persons with overlapping bodies adopted the new idiom I have just described would not entitle us to say they had overcome all difficulties about the communication of the content of those sensory experiences which they associated with their common part. For the difficulties which we ordinary people find in relation to views and sounds and smells would arise for them in relation also to pains considered as common perceptual objects. Either might say of the other, 'I wonder whether his experience when he feels one of our common pains is just like mine. Admittedly we can usually agree in our application of words like "long", "short", "throbbing", and "intense", but that does not prove that the content of his experience is the same as the content of mine.' It will be noticed that in this context the word 'feel' inevitably suffers a shift of meaning corresponding to that suffered by the word 'pain'.

Within the space which he has allowed himself Professor Kraft could not hope to deal fully with all the topics discussed in the Viennese Circle, and specialists may be disappointed by his treatment of some interesting items

such as the philosophy of mathematics and the theory of probability. But his apportionment of space seems to represent fairly the distribution of interest in the Circle, and anyone who reads the book carefully should gain a clear understanding of the reasons why the work of the Circle is still important. Of all philosophers on this side of the iron curtain the Germans are probably those who most need such understanding: it is to be hoped the book will have a wide circulation among them.

WILLIAM KNEALE.

Methods of Logic. By WILLARD VAN ORMAN QUINE, Professor of Philosophy, Harvard University. Henry Holt and Co., New York. Pp. xx + 264.

IN his new book on the elements of symbolic logic, Dr. Quine sets before himself two main aims: precise exposition of fundamentals, and development of deductive techniques that will help readers in their own logical work. These aims are attained to a degree that makes this one of the very best books of its kind.

The Introduction is marred by some crude epistemological discussion that is best skipped (we read, e.g., that we can find out if a statement is true by comparing it with the world 'or, which is the nearest we can come, by comparing it with our experience of the world'). But some of Quine's points could hardly be overstressed. First, logic is a scientific activity, concerned with truth; Quine has no use for the idea now prevalent that logic is a game, differing from chess or draughts only in being—Lord knows how—interpretable and applicable. Secondly, logic can deal with quite commonplace statements, and that without our having first to provide a complete analysis of them; all that is necessary is that statements and terms shall not change their sense and reference during a deduction. Thirdly, logic essentially deals with those statements whose truth is a result of their 'structure in terms of logical words'. Logic thus has no special subject-matter, and is not concerned to make specific statements, but rather to exhibit valid statement-patterns like 'if p and q, then p' (which may equally well be regarded as patterns of valid inference). Mathematics, on the other hand, is concerned to make specific statements about a special subject-matter, e.g. 'there is an even prime'; and therefore (even if we choose to call it 'logic') cannot be reduced without remainder to logic in the above sense of the word.

Parts 1 and 2 deal respectively with truth-functional logic and the logic of singly quantified statements. In both these fields, as is well known, the decision problem is soluble; Quine's achievement is to supply swift, elegant, and intuitively easy methods for discovering whether a formula is valid or consistent. He avoids all the usual mystifying talk about variables, propositions, and propositional functions; the schematic letters in formulae are explained simply as proxies for statements or predicates whose internal structure is at the moment irrelevant. He also eschews the term 'material implication', and tries to restore to the word 'implication' in logical discourse the familiar sense alone recognized by the OED. He usefully points out that a statement of implication is a statement *about* statements, whereas a conditional statement is not *about* its own antecedent and consequent but rather about what *they* are about. I rather regret the space devoted to a reformulation of the old logic of terms (§§ 12-15); learning to twist statements into the form of a copula between

two noun-phrases can be positively harmful to a student's logical perceptiveness; and Quine himself admits later on (p. 92) that such gymnastics are not really necessary.

Part 3 is devoted to the general logic of quantification. Here Quine shows great virtuosity. The rules of inference are so described that checking for validity is an easy mechanical task; *finding* a proof will call for ingenuity, and here Quine gives the sort of help that a novice at chess would get from a good book on chess strategy and tactics. There are four specially important moves, labelled UI, EG, UG, and EI. Of these, UI and EG are familiar; UI is the transition from a general statement to a singular instance, and EG is the transition from a singular instance to the corresponding existential statement. Quine rather exaggerates the novelty and strangeness of UG and EI; both are in fact quite natural steps. UG is the step whereby a schoolboy, having proved something of 'the triangle ABC', boldly concludes that the same holds good of *any* triangle. And EI is the procedure of passing from a premise to the effect that so-and-so holds in *some* case to the supposition that it holds in *this named* case; we find this procedure in Aristotle's *Prior Analytics*, where it is called ἔκθεσις. (Thus, to establish the validity of *Disamis* one would proceed thus. 'Suppose: (i) something that is M is P, (ii) everything that is M is S. Let us say (iii) x is M and x is P (ἔκθεσις from (i)). Then x is M; and, by means of *Barbara*, this and (ii) together yield: (iv) x is S. So x is S and P; therefore (v) something that is S is P. q.e.d.' A comparison of this with Quine's examples of EI will show that EI is essentially ἔκθεσις.)

The difficulty about UG and EI is that intuition supplies no foolproof rules for using them. Quine's great merit is to have produced mechanical tests for eliminating illegitimate uses. Arbitrary and artificial as these look, the possible fallacies cited by Quine show that in fact they are needed, and he has rigorously established the validity of any proof that passes the tests.

The jargon of 'free and bound variables' is not introduced till pp. 127-8; having done without it thus far, Quine might with advantage have dispensed with it altogether. The use of the term 'variable' almost inevitably leads to nonsense sooner or later; in Quine himself we get the nonsense 'To be is to be a value of a variable' (p. 224).

In Part 4, §§ 33-36 are more philosophical and less technical than the rest of the book. In § 33 Quine emphasizes that there are ostensible proper names without reference, like 'Cerberus' and '0 : 0'; and that this truth cannot be evaded by saying that such names stand for ideas, or meanings, or unrealized possibilities, or entities in some 'other' universe. About this I heartily agree. But Quine fails to make Frege's distinction between inapplicable predicates and empty names; he wrongly assimilates 'there is no such thing as Cerberus' and 'there are no Bolivian battleships'. In the latter statement there is a noun-phrase used predicatively, as we see in the equivalent statement 'nothing is a Bolivian battleship'; and a logical predicate can still properly be used even when it cannot be *affirmatively* predicated of anything. But a pagan who became a Christian would just stop using 'Cerberus' as a proper name. He would not be so using it in saying there is no such thing as Cerberus; for this is a disguised *mention* of the name 'Cerberus'; what it means is that 'Cerberus', as used by pagans, is not a name of anything.

This same confusion between names and predicates (referential and attributive expressions) runs through all that Quine says in these sections about proper names, descriptions, and variables; it is clear that he is

influenced by the pseudo-Aristotelian logic of terms. I shall not try to fit his assertions together; we find him apparently saying: (i) that variables of quantification can be substituted for proper names (pp. 83-4, 207); (ii) that they cannot be substituted for predicate expressions (pp. 206-7); (iii) that proper names are really predicates (p. 218)!

Quine's view of identity is also open to criticism. Because a statement of identity does not mean that its own right and left sides are identical expressions, Quine infers that it must assert a relation between objects. *Pari ratione*, since it was St. Peter, and not his name, that was so called after a rock, we must say that 'so called after a rock' is a predicate true of St. Peter—and why then is it not also true of Simon-bar-Jona? The truth is that, although it would be wrong to put 'Peter' in quotes in 'Peter was so called after a rock', nevertheless this statement is exponible as a statement *about* the name 'Peter'; it means that this name was given to somebody after a rock. And similarly 'Cicero = Tully' is exponible as a statement *about* these two names—viz. that somebody was called by both names. '$x = y$' is no more admissible in logic than 'x was so called after a rock' or 'x is an animal and x begins with D'. Mathematical uses of '=' do not presuppose a relation of identity; '$x + y = y + x$' means that a certain number both results from adding y to x and results from adding x to y, not that two sums stand in the relation of identity.

Whatever we may think of Quine's solutions, it is certain that mathematical logic cannot ignore the philosophical problems he discusses.

The last part of the book (§§ 37-41) is an excellent presentation of elementary set theory. One point of detail urgently needs revision. Quine uses selections on p. 234 (to define cardinal multiplication) in a way that might easily make the reader think the existence of selections is a matter of course (and the axiom of choice is nowhere mentioned). A beginner has a right to be warned of pitfalls here; he might, *e.g.*, be referred to *Introduction to Mathematical Philosophy*. (The existence of selections can indeed be proved, without the axiom of choice, for the finite cases that Quine is here primarily interested in; but this theorem —*120.62 of *Principia Mathematica* vol. ii—is far from elementary.)

P. T. GEACH.

Logika. By TADEUSZ CZEŻOWSKI. Warszawa: Panstwowe Zaklady Wydawnictw Szkolnych, 1949. Pp. 273. zl. 600.

THIS is a text-book of logic by the Professor of Philosophy in the University of Torun, formerly of the University of Wilno. It is competent, clearly written, and fairly comprehensive. It deals mostly with deductive logic, but includes also a chapter (about forty pages) on probability and induction, based to a large extent on Reichenbach's *Wahrscheinlichkeitslehre*. There is also a sketch of the history of logic. This is interesting and, on the whole, accurate, though the importance of the work of Frege is not sufficiently stressed. The treatment of the logic of relations is rather fuller than is usual in an elementary text-book, and should, I think, be especially useful. On the other hand, some of the topics which are mentioned (*e.g.* the intuitionistic calculus of Heyting) seem to me to be too specialised for a work of this kind.

So far as the more philosophical aspects of the book are concerned, two points may be mentioned. First, on page 20 the author says that the sentence (A) "John believes that today is Saturday" should be formulated

more exactly as (B) "John has a belief the expression of which is the sentence 'Today is Saturday'". This clearly won't do, if only because the proposition expressed by (A) neither entails nor is entailed by the proposition expressed by (B): to see this it is enough to translate (A) and (B) into German. Secondly, in discussing Russell's paradox (pp. 16-17), the author contrasts the (simple) theory of types with the theory of "semantical categories"; he agrees that the two are formally similar, but seems to maintain that the second possesses an intuitive justification which the first lacks. This has also been maintained by Lesniewski and by Tarski (*cf.* Tarski's *Der Wahrheitsbegriff in den formalisierten Sprachen*, pp. 334-335); but the point seems to me to be doubtful and to require further discussion. Also, the author is misleading in talking as if the simple theory of types (or the theory of semantical categories) were universally acknowledged to provide a final solution of the paradox.

The book contains a fairly extensive bibliography, especially of books and articles in Polish. It is odd, however, to find no reference to the works of Chwistek.

C. LEWY.

The Physical Basis of Mind. A Series of Broadcast Talks by SIR CHARLES SHERRINGTON, E. D. ADRIAN, W. E. LE GROS CLARK, S. ZUCKERMAN, E. T. C. SLATER, W. RUSSELL BRAIN, WILDER PENFIELD, VISCOUNT SAMUEL, A. J. AYER and GILBERT RYLE. Oxford: Basil Blackwell. Pp. 79. 5s.

> "The cerebral cortex is the seat of the mental processes known as feeling and volition. Feeling or eonsciousness is the final phase of sensory stimuli, and volition is the starting point of motor activity. The correlation of sensations with one another and with volitional impulses constitutes thought."

THIS quotation is taken, not from the all-star symposium under review but from an unpretentious laboratory manual of dissection that is widely used to guide the young zoologist through the insides of vertebrates. Its authors rightly felt that structural details of flesh and bone should be animated wherever possible by some reference to function, and they do here for the brain no more than they would be prepared to do for the pancreas or liver. It is a quotation noteworthy for its matter-of-factness, and for its representing pretty faithfully the average biologist's belief that consciousness is something that in recent eons has come to be fitted into the apex of the so-called Reflex Arc. There is no evidence, anyhow, of worry and concern. But most of the scientists in this symposium are clearly very much concerned and worried, and what is on their minds is, briefly, this. As physiologists they can record the physical events that take place in sense-organ, nerve, and even brain when looking at a tomato or a table or some other ingredient of the philosophers' habitual sensory diet. Unfortunately, the record comes to an end at a certain level, that at which the tomato sense-data invoiced by the oscillograph are delivered to the subject that 'apprehends' them or otherwise takes them in. It may be that the Subject thinks fit to eat the tomato: an act of volition is performed, and quite suddenly the recording apparatus starts working again and nerve impulses are traced from the dorso-lateral areas of the precentral gyrus to the intrinsic and extrinsic muscles of the hand. If the person tastes the tomato and, finding it unripe, spits

it out, the same sort of physical changes occur (though along different nervous pathways), but here too, at a certain point, the record fails: the sensation of tasting, its appraisal, the recollection of tomatoes formerly eaten, the flooding into consciousness of the realisation that this is not all that a tomato should be, the act of will which initiates its casting out—these are nowhere acknowledged by intelligible tracings on the smoked drum or twitches of the galvanometer. All are acts of mind, and what, it is asked, can their "material substratum" be?

Scientists, it may be observed, are used to asking questions of just this sort: text-books are written on the Physical Basis of Digestion or of Muscular Action, and a person who buys them knows what to expect inside. Someone who wanted to be told the physical basis of digestion could be answered in several ways; one acceptable answer in half-a-dozen words would be "the degradative action of hydrolytic enzymes", and it could be added that the Seat of Digestion is the stomach and intestines. Could one answer a question about the physical basis of mind in similar language? The physical basis of Mind is the complicated electrical activities of nerve fibres; the Seat of mental processes, since metaphor allows them to be sessile, is the brain. Unfortunately, this sort of answer, unlike the one about digestion, is widely thought to give evidence of philosophical illiteracy; it is received not with gratitude but with judicial pursings of the lips. But if the physical basis of Mind can be the subject of a proper question (by which I mean a question whose answer could be acknowledged by the comment "Ah, thank you, now I see") then what other *sort* of answer could it possibly have?

Professor Ayer suggests one way in which the scientists can have their peace of mind restored them. Thinking of one sort or another is doubtless accompanied by a complex electrical commotion in the cerebral cortex and elsewhere in the brain. It might turn out that wishing or purposive deliberation is accompanied by one sort of commotion and affectionate thoughts by another, and an innate sense of the fitness of things prompts us to hope and assume that volition is associated with spiky, purposeful-looking action potentials and loving thoughts by more rounded ones. Supposing these to be recorded and classified, a complete mapping of the electrical activities of the brain might in due course enable us to tell what a person is thinking about and to predict what sort of thing he is about to do. This is by no means impossible in physical principle, and its successful outcome might well be reported in the pages of the *Journal of Physiology*. It would be a record of a correlation of observations, fiendishly difficult in practice, no doubt, but not in principle different from the association of a low blood sugar level with hunger or peripheral vasoconstriction with fright. Correlations of this sort can add up to a complete and consistent picture, but their mapping, Ayer says, does not raise any special *philosophic* difficulty; indeed, Ayer is in the slightly unsympathetic position of one who does not know what all the fuss is about. There is, I suppose, one qualification: when the physiologist takes a record of a fasting subject's blood sugar concentration, the entry "75 mg. per 100 ml." is in reality correlated, not with the so-called emotion of hunger, but with the patient's saying "I am hungry" or the record of his trying surreptitiously to eat a bun. Of course the patient might be lying, or might be only pretending to want a bun, but that is on all fours with the possibility that the physiologist was lying also. It is a disability for which a more widespread recognition of the virtue of truthfulness seems to be the only cure.

Ayer's commentary will be thought by the scientists to be no more, perhaps, than merely palliative, for they are the victims of much more grandiose and confused ambitions. What they really want to know is (*a*) what is the physical basis of the percipient Subject who earlier ate the tomato—the Subject to whom sensations are presented and who, by the metaphor's becoming almost painfully literal, is the occupant of the seat of mind? And (*b*), by what means (and where) does this Subject influence the body and *vice versa*: at present they seem to have the same sort of intimate but mutually ineffective interpenetration and coexistence as the subjects of two snapshots which oversight or misplaced frugality has caused to be taken on a single strip of film. Mind and body meet in Lord Samuel, "where mind accepts and utilises the sense data offered by the brain"; Dr. Russell Brain speaks of his mind's perceiving "patterns"; Professor Adrian asks "how a particular pattern of nerve impulses can produce an idea; or . . . how a thought can decide which nerve cells are to come into action"; and Professor Le Gros Clark puts the same question in a different way. Slater and Zuckerman alone are analytical and wary. It is curious that Adrian before the microphone should speak of his thoughts deciding his actions, though in the laboratory he would simply say he had decided something or come to a decision. Who or what has the thoughts that administer Adrian's decisions for him? Dr. Penfield's answer is that it is the switchboard operator of his CNS exchange; but Professor Ryle, having reviewed the possibility of there being two Professor Adrians, decides that one will do. Why could not Adrian have asked himself "What physical activities in my head accompany my seeing a tomato or my thinking hard about a course of action?" For this is the sort of question to which the rest of his contribution provides an (admittedly) rudimentary answer and which future Adrians, thanks to the labours of their prototype, will one day answer pretty fully. Ryle's answer to the scientists' gropings and misgivings about the authority and whereabouts of the Penfield telephonist is, briefly, that he simply isn't there. It is an answer that should bring a sense of relief to many. Since Ryle's contribution can be regarded as a footnote to *The Concept of Mind*, or *vice versa*, its arguments need no re-affirmation here.

Professional philosophers will find this book worth reading for two reasons: (*a*) it contains a lot of worthwhile information about the structure of the brain, epilepsy, prefrontal leucotomy, brain waves, and so on—information that might well be enlarged by Adrian's Wayneflete or Fulton's Withering lectures. Second, (*b*) it is a treasure-house of quotations suitable for setting as questions in the Schools. But before the young philosopher starts filing his teeth to get a better grip of them, it should be made clear that the confusions he is now to make short work of are the result of centuries of painstaking and almost unaided work by members of his own profession. What the lay public made of it all one can but wonder. Probably—to borrow a Wodehouse fancy—their ears were sensitive enough to detect the capital letter with which Mind was throughout distinguished; probably they just heard Ayer's inverted commas; but as to Ryle's exclamation mark, probably not.

P. B. MEDAWAR.

Value: A Cooperative Enquiry. Edited by RAY LEPLEY. Columbia University Press (London: Geoffrey Cumberlege), 1949. Pp. x + 487. 40s.

IT is a tempting thought that if only someone would formulate clearly the fundamental problems in Moral Philosophy, and a number of thinkers of different schools give their answers to them, the whole subject would leap forward into a clearer air. It is fitting, moreover, that the attempt should be made in America, where all shades of opinion are well represented. The question-master in this brains-trust is Professor John Dewey, who, in the *Journal of Philosophy* for 1944, set out what he considered to be the basic questions for "value-theory", and challenged all and sundry either to answer them, or else to say what questions they would substitute as the basic ones. Out of this challenge the present volume originated. It is a collection of papers by fourteen philosophers, including Dewey himself, which are the culmination of a great deal of discussion between the contributors. The papers are preceded by Dewey's original article, and followed by 180 pages of mutual criticisms and rejoinders. The participants, besides Dewey and the editor, are H. D. Aiken, C. E. Ayres, A. C. Garnett, G. R. Geiger, L. E. Hahn, B. E. Jessup, H. N. Lee, E. T. Mitchell, Charles Morris, D. H. Parker, S. C. Pepper, and P. B. Rice.

To be fair to all these writers in a short review would be a formidable task; but some generalisations are possible which seem to apply to most of the contributors, though not to the same degree. For they seem—whether because of the way in which Dewey started the discussion off, or because they are all more or less his disciples—to approach the subject with very much the same set of presuppositions as each other. This is not to say that there is no genuine controversy in the book, but simply that the really fundamental questions have already been answered implicitly before the discussion begins.

The contributors' common presuppositions are illustrated by the frequent occurrence throughout the book of such expressions as "value-facts", "value-events", "value-phenomena", "value-data" and "value-objects". Thus "values" are conceived of as a class of entities existing in the world, the problem being to say what they are, tell us how to recognise them, and give some account of their nature. Only thus, think most of the writers, can value-theory be made "scientific". To be "scientific" means to have a definite subject matter accessible to methodical observation. This position is put most clearly by Morris (p. 211), in the course of his plea for a "scientific axiology", or "science of preferential behaviour":

> A science proceeds by selecting a subject-matter for study, by developing a terminology to talk about this subject-matter, and by formulating and verifying statements made in this terminology about the subject-matter chosen for investigation.

Similarly Aiken says (p. 19):

> The cause we plead is the cause of a science of valuation, which is possible only if our value-terms are given a definite descriptive meaning,

and Ayres, in a verdict which seems to be its own death-warrant, says (pp. 58 f.):

> We must be prepared to declare without fear and without scorn that nothing but science is true or meaningful; we must do so because it is a fact. . . . Any proposition which is incapable of investigation by scientific methods is meaningless and worthless as meaning and value are conceived in that universe of discourse.

Thus the common purpose of most of the writers seems to be to rescue value-judgments from the frightful accusation that they are "unscientific"; and this seems to them to involve saying clearly what "value" is, and showing that judgments about it are not different—except by having different subject-matter—from the records of ordinary scientific observations. The controversy is mostly about the merits of rival theories as to what "value" is. Some say that it is something "behavioural", some that it is something "mentalistic"; Lepley, as befits an editor, says (p. 169):

> In this broader sense it appears likely that the behavioural and the subjective are but two different ways of viewing and stating the same total interactive and transactive processes which constitute value-events.

But it is nowhere indicated what criteria there could be for deciding between these various views.

As we should expect from those who adopt such an approach, the material mode of speech is widely used, and leads to familiar confusions. For example, the problem raised by the emotivists as to whether reasoning about values is possible—a problem which is a logical one, and settlable by studying whether value-judgments are entailed by other judgments or by one another—is stated by Dewey in the following way (p. 11):

> Are value-facts bias-facts of such intensity and exclusiveness as to be unmodifiable by any possible consideration of grounds or consequences?

This looks like an empirical question: but by what sort of observation could it be settled? Again Geiger (p. 96) puts a subjectivist position by saying "Values are the products of human choices". This looks like an empirical statement of the same character as "Estuaries (a kind of observable entity) are the products of tidal action (a kind of observable process)". Thus logical problems about the way in which words like "good" are used are made to look like empirical problems about the way in which a stuff called "value" behaves or is constituted.

There is, however, quite a lot of talk about the necessity of finding "referents" for value-words. Garnett, for example, says (p. 79):

> Our first problem, therefore, is to search for the referents in cases of successful communication concerning values.

This, in so far as it directs attention to the problem of the way in which value-words are used, is better than asking "What is value?"; but a further crop of confusions is generated by the assumption that to study how words are used is pre-eminently to search for their "referents", *i.e.* to ask to what things people apply them (or should in the opinion of the writers apply them). This would be a mistake even with words like "brown"—for many other things can be learnt about the logic of this word beside the fact that it is applied to a certain distinguishable class of objects—but with words like "good" it is fatal. How fatal it is can

be seen by supposing the programme carried out; let us imagine a group of "axiologists" who want to make their evaluations "scientific", searching systematically for a "referent" for the word "good". They see to what objects the word is applied by their neighbours (as might be a sensible procedure if the word they were studying were "brown") and they decide which of these applications they themselves consider proper (and this will obviously depend on their own standards of values). After doing enough of this, they publish their generalisations, which inevitably amount to this, that "good" *means* or stands for a lot of various things which are valued by their neighbours and their society in general and themselves in particular. Similar results would be achieved if, instead of studying only the applications of the *word* "good", they studied all kinds of "preferential behaviour", to see what people *thought* good.

Then suppose that the same group of "axiologists", in search of guidance in this perplexing life, having armed themselves with these findings about the meaning of "good", get down to the main job of finding out, by "scientific evaluation", what things *are* good. Their task will be easy, and can lead only to one result; they can rest content with their society's way of life—especially their own version of it—for all the things that they and their neighbours generally value are good; that is what "good" means, just those very things. If you establish the meaning of "good" by seeing to what it is applied, then you will be forced to the conclusion that what is called good *is* good—especially what you yourself call good. If Coca-cola is put in at one end of this machine, Coca-cola will flow out at the other: and if vodka, vodka. But this does not help us to evaluate either Coca-cola or vodka—only to find out how certain people evaluate them.

There is another trap into which followers of such methods readily fall. If you are looking for a "referent" for a value-word, you may think you have found one when you have given a definition of it in which the definiens appears to consist of words having an unambiguous descriptive meaning. But unless the definition is obviously inadequate, there will nearly always be found some word in the definiens which is itself a value-word. Thus the word "satisfaction" frequently appears in this role; but how would we verify by observation the sentence "I am satisfied with so-and-so"? Parker says (p. 226) that "value is a new flash of satisfaction, shooting up in one's own or someone else's centre"; but how should we determine when this phenomenon had occurred? Aiken, who is inclined to define "value" as "satisfaction" says of this latter (pp. 40 f.):

> It is activity which is generally free from *irreconcilable conflict*, free from *frustration*, free from *want* in the merely *privative* sense, though certainly not free from *interest*.

My italics indicate what a nest of evaluations lies concealed within this so-called description, and how far Aiken is from giving to his value-terms "a definite descriptive meaning". For what is the empirical difference between "want in the merely privative sense" and "interest"?

This and other confusions would be avoided if more attention were paid to the question, "What is the logical character of value-words?", in order to determine in what respects they behave like other classes of words, in what respects differently. This demands the most painstaking study of actual usage, followed by such logical signposting as is required to help us avoid those sophistical pitfalls into which the uncritical use of language may lead us. To assume without sufficient examination that

"good" is like "brown" is as bad as to assume that "this" is like "brown", or that "exists" is like "growls", or that "all tigers" is like "tame tigers", or that "know" is like "see". To assume that if "good" is not like "brown" it cannot be used "scientifically" is like assuming that if "this" is not like "brown" it cannot be used "scientifically", or that if "Greenwich" is not like either it cannot be used "scientifically" (in fact, that all words had better do the same sort of job, or so much the worse for them).

I do not want to suggest that the views which I have been attacking are held by all the contributors, or even that no protests are made against them in the book itself. Some very sensible things are said from time to time, especially by Parker; but whether from respect for Dewey, or out of natural politeness or co-operativeness, no one ever seems to say incisively what is wrong with the whole enquiry; and this must be the reviewer's excuse for saying it as incisively as he can.

R. M. HARE.

Evolution and the Founders of Pragmatism. By PHILIP WIENER. Cambridge: Harvard University Press, 1949. Pp. 288. Price in Great Britain 40s.

THIS is an important book which, despite its alarming cost in this country, no serious philosophical library should be without. It is based, Professor Wiener tells us, on a series of investigations into the origins of American Pragmatism, and is made up largely of studies of six of the most distinguished members of Peirce's Metaphysical Club which met very rarely and informally at Harvard in the 1860s and '70s and which, unlike its English counterpart of the same name and period, led to the hatching of some very important ideas.

Of the importance of Professor Wiener's subject and the scrupulous accuracy of his scholarship, there can be no question; the weaknesses of his book—or what seems so to the present reviewer—are perhaps endemic to its *genre*, the history of relatively recent ideas. Historical treatment of any movement of thought means an attempt at objective treatment; yet it is almost as difficult to write objectively of a movement as recent as Pragmatism as it is to think objectively about the standards and motives of one's parents or grandparents. Professor Wiener writes as a distinguished inheritor of the 'pluralistic empiricism', 'temporalism', 'relativism', 'fallibilism' and 'secular democratic individualism' of the great men whose thought he analyses; yet the greatest of them, by philosophical standards, does not rest very happily in this nest of 'isms'. A second weakness is that, despite the title of his first chapter, Professor Wiener hardly mentions the specifically American background of Pragmatism, in particular the Transcendalist movement of the 1840s and '50s which left its mark indelibly on the work of, at any rate, Peirce and James. A third weakness, altogether typical of essays in intellectual history, is that much of Professor Wiener's most important material is to be found, not in his text, but in his voluminous notes and appendices.

Professor Wiener's six thinkers are Peirce, James, Chauncey Wright, Nicholas Green, John Fiske and Oliver Wendell Holmes. Each of these thinkers was profoundly impressed by the Darwinian controversy, and each, Professor Wiener claims, tried to elaborate the philosophical significance of evolution for his own chosen branch of studies. The thinkers

to which this thesis applies most clearly are Chauncey Wright and William James, and Professor Wiener deals with them excellently. To most students of philosophy in this country Wright is known, through Peirce and James, simply as J. S. Mill's Harvard spokesman in the 1860s and '70s. Professor Wiener's study reveals a very different character: unlike Mill, Wright was a man of science through and through, an able mathematician, the first of Darwin's disciples to treat mathematically certain aspects of the process of natural selection, and a really first-class writer. He resembled Mill only in his grasp of the ethical neutrality of science and in his sane but enthusiastic liberal idealism. Two of Wright's best dicta, one on ethics and one on philosophical method, may do something to show the quality of a mind which Professor Wiener has presented with such just admiration and insight. "All the ends of life are within the sphere of life . . . (which) is neither good nor evil but the theatre of all possible goods and evils." "The most profitable discussion is, after all a study of other minds—seeing how others see, rather than the dissection of mere propositions."

In his chapter on James Professor Wiener claims to do little more than go over the ground which Professor R. B. Perry has covered in his *The Thought and Character of William James*. But here Professor Wiener does himself less than justice. If this chapter called for no new research, its main threads are held together with great skill, and its general assessment of the influence of Darwinian ideas both on the *Principles* and on James' philosophical writings could hardly be bettered. "James in his metaphysics dramatised the external world of sensations and the inner world of rational, moral, æsthetic, and religious sentiments." This judgment helps us to understand both the enduring merits of James the pyschologist and the essentially temporary message of James the philosopher.

The two chapters on the lawyers, Green and Holmes, are of great interest; but here the connexion between Professor Wiener's two main themes, pragmatism and evolution, is a little hard to follow. He shows clearly that both Green and Holmes understood and appreciated Peirce's logical pragmatism; but he does almost nothing to show that their distinctively practical and historical attitude to legal problems is at all closely connected with such evolutionary conceptions as stem from Darwin. The chapter on the Unitarian teacher, John Fiske, provides some useful background material; but Fiske is clearly the weak brother of Professor Wiener's party, and it is a pity that no chapter is devoted to Ellingwood Abbot, who converted Peirce to 'scholastic realism' and was, by all accounts, one of the strongest thinkers of the Metaphysical Club.

It is Professor Wiener's treatment of Peirce that, in the present reviewer's opinion, is least satisfactory. Peirce, notoriously, is among the most erratic of the world's great philosophers. This is perhaps particularly true of his writings on evolution in which astonishingly accurate insights and prophecies can be found side by side with statements that to the unsympathetic reader may appear downright absurd. Now Professor Wiener has no desire to 'guy' Peirce: on the contrary he shows in a number of passages a profound and courageous appreciation of the importance and scope of Peirce's evolutionism. But the general effect of his book is to give a picture of Peirce as at best a brilliant crank, at worst a crackpot polymath, whose wild theories included a surprising number of guesses which proved to be correct. More specifically Professor Wiener would surely have done well to have laid more emphasis on the following points: (1) Peirce's evolutionism is not an attempted application of Darwinian

ideas; its origin must be traced to Peirce's readings in the American Transcendentalists, in Schiller's Aesthetic Letters (which he read—how wisely—before embarking on Hegel) and in his profound appreciation of certain doctrines of Scotus, Aquinas and Aristotle. Darwin, it seemed to Peirce, had sinned by presenting his magnificent array of evidence for the existence of evolutionary processes and for natural selection as their main mechanism within a hopelessly inadequate, 'Nominalist' or 'Ockhamist' framework of ideas. (2) Peirce had at least the merit of distinguishing very clearly between the fact or story of evolution—something as undeniable as the more general fact that the world contains processes of telelogical character—and the various theories as to the causes of evolution which have been propounded. In this respect he scores over most other evolutionary cosmologists and indeed over most philosophers of biology. (3) Peirce was the first philosopher to call attention to the logical significance of Darwin's postulate of a tendency to random variation in the offspring of all breeding groups, and to point out that this makes the Darwinian theory into a kind of skeleton statistical theory. Peirce's importance as a metaphysician lies largely in the fact that he took this point seriously. (4) Although Peirce's writings on Darwin are often biased by an excessive emphasis on points (2) and (3) above, he never denied that natural selection is the most important, because most obviously traceable, cause of biological evolution. He did insist, however, that the general evidence suggests the operation of causes of other types as well, *e.g.*, cataclysmic changes in the physical environment of the forms in question, and changes originated by sheer sporting and maintained by processes roughly akin to those of learning. (In this latter way he hoped to keep the kernel of truth that "lies enwrapped in the theory of Lamarck".) To good Darwinians of the 1880s and '90s these suggestions must have seemed wickedly reactionary; but after the passage of sixty odd years it is difficult not to admire the level-headedness and breadth of vision which they evinced.

But if Professor Wiener is unjust on these scores to the great central figure of his story, he makes some amends elsewhere. He quotes most usefully from a number of hitherto unpublished manuscripts in the Widener Library, and he summarises, as an appendix, a long paper dated 1901, entitled *Hume on Miracles and Laws of Nature* which certainly deserves to appear in any final edition of Peirce's works. He also throws a very interesting light on Peirce's early philosophical interests, plans and projects, which were as grandiose and in the main as uncompletable as those of his mature years. In these ways Professor Wiener has done much to add to our understanding of a prodigious genius who, by all accounts, was in life an impossible person to deal with, but who to-day, alone of nineteenth-century philosophers, is first-rate philosophical company, Peirce, who knew so much, and *did* so much, and from whom contemporary philosophy has still so immensely much to learn.

W. B. GALLIE.

De Thalès à Bergson. By P. DEVAUX. Liège, Sciences et Lettres, 1949, Pp. 439. About 35s.

As the author states in his preface this is an "*initiation philosophique*" based on an introductory course of lectures in the history of philosophy. Hence it consists mainly of outlines of the principal doctrines of the chief philosophers up to the end of the nineteenth century, and the author

attempts to trace certain main currents of thought and show the influence of one philosopher on another. His emphasis is on metaphysics and theory of knowledge, though he devotes some pages to the details of the Aristotelian syllogism and discusses briefly the ethical views of Kant and a few others. The work aims primarily at sympathetic exposition but some simple criticisms are given as well. Although necessarily sketchy it is on the whole reasonably clear and sound, but one may find fault with the relative importance he attaches to the different philosophers. Aquinas, Locke, Berkeley and Hume come off badly, while Bergson and some nineteenth century philosophers get an undeservedly large share of space; thus only three pages are devoted to Locke's *Essay*, whereas the Pythagoreans get six and Comte's doctrines seventeen. Nor must it be supposed from the title that the author carries his history up to the death of Bergson; with an occasional minor exception he stops at about 1910, and so the lack of any discussion of the important developments in philosophy in the last forty years seriously detracts from the value of this work as an introduction for students.

The book includes short biographies of the philosophers discussed and contains full indexes of names and topics.

R. J. Hirst.

Received also :—

G. Bachelard, *L'Activité Rationaliste de la Physique Contemporaine*. Paris, Presses Universitaire de France, 1951, pp. 223, 500 fr.

K. Bhattacharyya, *Object Content and Relation*. Calcutta, Published by the Author at Das Gupta & Co. Ltd., 1950, pp. 160.

G. Boas, *Wingless Pegasus : A Handbook of Art Criticism*. Baltimore, The Johns Hopkins Press (London : Geoffrey Cumberlege), 1950, pp. x + 243, £1 8s.

Emile Brehier, *Chrysippe et l'Ancien Stoïcisme*. Paris, Presses Universitaires de France, 1951, pp. 276, 600 fr.

A. Burlond, *De la Psychologie a la Philosophie*. Paris, Librairie Hachette, 1950, pp. 138.

M. Chastaing, *L'Existence d'Autrui*. Paris, Presses Universitaires de France, 1951, pp. 362, 800 fr.

Condillac. Édition Georges le Roy. Tome XXXIII of *Auteurs Modernes*. Paris, Presses Universitaires de France, 1951, pp. xii + 604, 1.800 fr.

R. Daval, *La Valeur Morale*. Paris, Presses Universitaires de France, 1951, pp. 201, 400 fr.

R. Daval, *La Métaphysique de Kant*. Paris, Presses Universitaires de France, 1951, pp. 400, 800 fr.

J. Delvolvé, *La Fonction Morale*. Paris, Presses Universitaires de France, 1951, pp. 146, 240 fr.

A. P. D'Entrèves, *Natural Law* (Hutchinson's University Library : Philosophy). London, 1951, pp. 126, 7s. 6d.

O. N. Derisi, *La Persona*. Su esencia, su vida, su mundo. La Plata, Ministerio de Educacion, 1950, pp. 394.

G. Faggin, *Schopenhauer*. Firenze, La Nuova Italia, 1950, pp. viii + 292, L. 1000.

D. Faucci, *Storicismo e Metafisica nel Pensiero Crociano*. Firenze, La Nuova Italia, 1950, pp. xi + 162, L. 800.

Golden Jubilee Souvenir 1895–1946. 2nd edition. Sri Ramanasramam. Tiruvannamalai, S. India, Sri Niranjanananda Swamy, 1949, pp. xiv + 376, £1 16s.

A. B. Gordon, *A Pint of Philosophy*, Dexter, Maine, by the author, 1951, pp. 30.

T. A. Goudge, *The Thought of C. S. Peirce*. Toronto University Press (London: Geoffrey Cumberlege), 1950, pp. xii + 360, £2.

G. Gusdorf, *Mémoire et Personne*. 2 vols: La Mémoire Concrète and Dialectique de la Mémoire. Paris, Presses Universitaires de France, 1951, pp. 563, 600 fr. each vol.

D. Hilbert and W. Ackermann, *Principles of Mathematical Logic* (translation). Edited by R.E. Luce. New York, Chelsea Publishing Co., 1950, pp. x + 170.

T. E. Hill, *Contemporary Ethical Theories*. New York, The Macmillan Co., 1950 pp. xii + 368, £1 10s.

J. Holloway, *Language and Intelligence*. London, Macmillan & Co. Ltd., 1951, pp. xv + 192, 12s. 6d.

O. Janssen, *Seinsordnung und Gehalt der Idealitäten*. Meisenheim/Glan, West Kultur Verlag Anton Hain, 1950, pp. 284, D.M. 13.50.

J. Katz, *Plotinus' Search for the Good*. New York, King's Crown Press (London: Geoffrey Cumberlege), 1950, pp. ix + 106, 16s.

G. Kränzlin, *Existenzphilosophie und Panhumanismus*. Schlehdorf am Kochelsee, Obb, Bronnen-Verlag, 1950, pp. 250.

C. Lamont, *Humanism as a Philosophy*. New York, Philosophical Library, 1951, pp. xxii + 368, $3.75.

L. Lavelle, *Traité des Valeurs*. Paris, Presses Universitaires de France, 1951, pp. xv + 744, 1.200 fr.

E. Leroux and A. Leroy, *La Philosophie Anglaise Classique*. Paris, Librairie Armand Colin, 1951, pp. 214.

H. D. Lewis, *Morals and Revelation*. London, George Allen & Unwin Ltd., 1951, pp. vii + 258, 16s.

R. C. Lodge, *Applied Philosophy*. London, Routledge & Kegan Paul Ltd., 1951, pp. xi + 243, 14s.

Logic and Language. Ed. by A. G. N. Flew. Oxford, Basil Blackwell, 1951, pp. vii + 206, 16s.

Maïmonides. Sa vie, son Oeuvre avec un Exposé de sa Philosophie by H. Sérouya. Paris, Presses Universitaires de France, 1951, pp. 152, 120 fr.

D. J. McCracken, *Thinking and Valuing*. London, Macmillan & Co. Ltd., 1951, pp. ix + 238, 15s.

Howard C. McElroy, *Modern Philosophers: Western Thought since Kant*. New York, Russell F. Moore Co., 1950, pp. xii + 268, $4.00

F. Meglio, *La Filosofia dell'Infinito*. Milan, Fratelli Bocca Editori, 1951, pp. 227, L. 650.

J. Moreau, *Réalisme et Idéalisme chez Platon*. Paris, Presses Universitaires de France, 1951, pp. 135, 300 fr.

Enzo Paci, *Studi di Filosofia Antica e Moderna*. Turin, G. B. Paravia & C., 1951, pp. 248, L.600.

W. van Orman Quine, *Methods of Logic*. New York, Henry Holt & Co., 1950, pp. xvii + 264.

W. H. V. Reade, *The Christian Challenge to Philosophy*, London, S.P.C.K., 1951, pp. xiii + 194, 13s. 6d.

E. Rogge, *Axiomatik alles Möglichen Philosophierens*. Meisenheim/Glan, Westkulturverlag Anton Hain, 1951, pp. xv + 240, D.M. 11.50.

J. Oliver Russell, Jr., *The Philosophies of F. R. Tennant and John Dewey.* New York, Philosophical Library, 1950, pp. xvii + 516, $6.00

Jean Paul Sartre, *The Psychology of Imagination.* London, Rider & Co., 1951, pp. vi + 222, 15s.

K. L. Saw, *The Vindication of Metaphysics.* A study in the Philosophy of Spinoza. London, Macmillan & Co. Ltd., 1951, pp. 173, 12s. 6d.

K. Schilling, *Geschichte der Philosophie.* Vol. I. Die Alte Welt. Das Christlich-germanische Mittelalter. München/Basel, Ernst Reinhardt Verlag, 1951, pp. 455, S.Fr. 20.

E. P. Shahan, *Whitehead's Theory of Experience.* New York, Kings Crown Press (London: Geoffrey Cumberlege), 1950, pp. viii + 140, 16s.

F. L. di Spadafora, *La Conoscenza Come Approssimazione.* Padua, CEDAM, 1951, pp. 112, L. 650.

Hippolyte Taine, Sa vie, son oeuvre avec un Exposé de sa Philosophie by A. Cresson. Paris, Presses Universitaires de France, 1951, pp. 151, 120 fr.

Manfred Thiel, *Ontologie der Persönlichkeit.* Erster Band. Die Kategorie des Seinszusammenhanges und die Einheit des Seins. Berlin, Springer-Verlag, 1950, pp. xxiii + 635, D.M. 29.70.

D. H. Th. Vollenhoven, *Geschiedenis der Wijsbegeerte* I. Uitgever, T. Wever Franeker, 1950, pp. 618, Du. Guilders 25.

Hermann Wein, *Das Problem des Relativismus.* Berlin, Walter de Gruyter & Co., 1950, pp. 126, D.M. 7.50.

M. Weitz, *Philosophy of the Arts.* Harvard University Press (London: Geoffrey Cumberlege) 1950, pp. xi + 239, £1 5s.

H. K. Wells, *Process and Unreality.* A Criticism of Method in Whitehead's Philosophy. New York, Kings Crown Press (London: Geoffrey Cumberlege), 1950, pp. xiv + 211, £1.

R. Bossard, *Psychologie des Traumbewusstseins.* Zurich, Rascher Verlag, 1951, pp. vii + 419, D.M. 19.80.

A. Cannon, *The Science of Hypnotism.* London, Rider & Co., 1951, pp. 126, 7s. 6d.

J. Dollard and N. E. Miller, *Personality and Psychotherapy.* London, McGraw-Hill Publishing Co., 1950, pp. xiii + 488, £2 15s. 6d.

J. J. Gibson, *The Perception of the Visual World.* Boston, Houghton Mifflin & Co., 1950, pp. xii + 235.

L'Année Psychologique publiée par H. Pitron, A. Fessard and P. Fraisse (1948). Paris, Presses Universitaires de France, 1950, pp. 700, 1.800 frs.

S. Lorand, *Technique of Psychoanalytic Therapy.* London, George Allen & Unwin Ltd., 1951, pp. vi + 251, 12s. 6d.

J. Maisonneuve, *Que sais-je? Psychologie Sociale.* Paris, Presses Universitaires de France, 1951, pp. 128.

A. Ombredane, *L'Aphasie et l'Elaboration de la Pensée Explicite.* Paris, Presses Universitaires de France, 1951, pp. 440, 1.000 fr.

P. Polatin and E. C. Philtine, *How Psychiatry Helps.* Kingswood, Surrey, The World's Work (1913) Ltd., 1950, pp. xii + 249, 10s. 6d.

Otto Rank's *Seelenglaube und Psychologie* (Psychology and the Soul). Trans. W. D. Turner. Philadelphia, University of Pennsylvania Press (London: Geoffrey Cumberlege), 1950 pp. viii + 195, £1 8s.

J. B. Rhine, *Telepathy and Human Personality.* The 10th Frederic W. H. Myers Memorial Lecture, 1950. London, The Society for Psychical Research, 1951, pp. 39, 1s. 6d.

Thomas W. Salmon, Psychiatrist. By Earl D. Bond. New York, W. W. Norton & Co. (Great Britain: Chapman & Hall Ltd.) 1950, pp. 237, 15s.

Warner Allen, *The Uncurtained Throne.* London, Faber & Faber Ltd., 1951, pp. 217, 12s. 6d.

L. Collis, *Without a Voice.* London, Faber & Faber, 1951, pp. 245, 10s. 6d.

C. A. Cooke, *Corporation Trust and Company.* Manchester University Press, 1950, pp. 206, 15s.

C. Darwin, *The Origin of Species.* Oxford University Press (Geoffrey Cumberlege) Worlds Classics (reprint of 6th edition) 1951, pp. xxxii + 592, 4s. 6d.

M.-M. Davy, *The Mysticism of Simone Weil.* London, Rockliff Publishing Corporation Ltd., 1951, pp. 84, 5s. 6d.

H. P. Fairchild, *Versus: Reflections of a Sociologist.* New York, Philosophical Library, 1950, pp. xvii + 203, $3.75.

H. P. Fairchild, *The Prodigal Century.* New York, Philosophical Library, 1950, pp. xvii + 258, $3.75.

Jean Guitton, *Essay on Human Love.* London, Rockliff Publishing Corporation Ltd., 1951, pp. xi + 243, 15s.

M. P. Hall, *Self-Enfoldment by Disciplines and Realization.* London, Rider & Co., 1951, pp. 159, 15s.

Health and Human Relations in Germany. New York, Josiah Macy, Jr. Foundation, 1951 (London: H. K. Lewis & Co. Ltd.), pp. 207, 5s.

The Hebrew Impact on Western Civilization. Edited by Dagobert D. Runes. New York, Philosophical Library, 1950, pp. xiv + 922, $10.00.

A. Hill, *Borderlands of Science.* New York, Philosophical Library, 1950, pp. viii + 424, $3.75.

H. Holland (compiler), *Why are you Single?* Kingswood, Surrey, The World's Work (1913) Ltd., 1950, pp. xi + 220, 9s. 6d.

Robert G. Ingersoll, The Letters of. Collected and edited by E. I. Wakefield. New York, Philosophical Library, pp. xii + 747, $7.50.

Dallas Kenmare, *Stolen Fire.* A Study of Genius. London, Williams & Norgate Ltd., 1951, pp. 152, 8s. 6d.

K. Mannheim, *Freedom, Power and Democratic Planning.* London, Routledge & Kegan Paul Ltd., 1951, pp. xxiv + 384, £1 5s.

Medieval and Renaissance Studies, Vol. II, edited by Richard Hunt and Raymond Klibanksy. University of London Warburg Institute, 1950, pp. 189, £1 1s.

C. Morgan, *Liberties of the Mind.* London, Macmillan & Co. Ltd., 1951, pp. vii + 252, 12s. 6d.

Swami Narayanananda, *The Primal Power in Man* or *The Kundalini Shakti.* Rishikesh (U.P.), N.K. Prasad & Co., 1950, pp. 155, Rs. 4/-.

Swami Narayanananda, *The Way to Peace, Power and Long Life.* 2nd enlarged and revised edition. Rishikesh (U.P.) N.K. Prasad & Co., 1950, pp. 190, Rs. 2/8.

J. Needham, *Human Law and the Laws of Nature in China and the West* (L. T. Hobhouse Memorial Trust Lecture). London, Oxford University Press (Geoffrey Cumberlege) 1951, pp. 44, 2s. 6d.

H. Pestallozzi, *The Education of Man. Aphorisms.* New York, Philosophical Library, 1951, pp. xii + 93, $2.75.

Viscount Samuel, *Essay in Physics.* Oxford, Basil Blackwell, 1951, pp. 154, 7s. 6d.

E. W. Sinnott, *Cell and Psyche. The Biology of Purpose.* North Carolina University Press (London: Geoffrey Cumberlege), 1950, pp. 121, 16s.

H. N. Spalding, *A Poem of Praise.* Oxford, Basil Blackwell, 1950, pp. viii + 71, 7s. 6d.

Bulletin de l'Université l'Aurore Série III, Tome 10, No. 40, October 1949. Shanghai, Don Bosco Industrial School Printing Press, 1949.

Journal of the History of Ideas, Vol. XI, No. 4, October 1950. Lancaster, Pa. and New York, Journal of the History of Ideas, Inc., 1950.

Etudes Blondéliennes. Directeurs J. Paliard and P. Archambault. Fasc. 1. Paris, Presses Universitaires de France, pp. 122, 300 frs.

Philosophia. Anno VII, Nuj. 13. Mendoza, Argentina, Instituto de Filosofia, 1950, pp. 114.

Proceedings of the British Academy 1946. London, Oxford University Press (Geoffrey Cumberlege) 1951, pp. x + 432, £2.

Proceedings of the Indian Philosophical Congress. Silver Jubilee Session. Calcutta, 1950. Pt. 1, pp. 153.

Question. People at Work. Edited by H. Westmann. Vol. 3, Nos. 1 and 2. Published 3 times a year for the Present Question Conferences Ltd. 7s. 6d. annually.